

# The Psychological Record

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## SELECTIVE ATTENTION AND THE MUELLER-LYER ILLUSION<sup>1</sup>

RILEY W. GARDNER and ROBERT I. LONG

*The Menninger Foundation*

The Müller-Lyer illusion has long served as a testing ground both for theories of illusions and for more general theories of perception. Earlier attempts at explanations of the illusion varied widely (see, e.g., Woodworth and Schlosberg, 1954). Recent major attempts have been notable for the different levels of behavioral organization at which they have been pitched. Spitz and Blackman (1958) have provided experimental evidence suggesting that cortical satiation phenomena (Köhler and Wallach, 1944) may be involved in such illusions. Motokawa (1950), however, has provided evidence that, in the visual modality, the Müller-Lyer and other illusions may be determined by the patterning of electrical excitation of the retina made by figures of different shapes and Nakagawa (1958) has confirmed and extended Motokawa's findings concerning the Müller-Lyer illusion. Piaget (Piaget, Mairé, and Privat, 1954; Piaget and von Albertini, 1950; Piaget, 1961) has explained the illusion as the resultant of relative "centration effects" among the lines and spaces involved in the figure. Gardner (in press) has shown that a dimension of cognitive control called Field-Articulation, relevant to the selectivity of attention deployment, may in part account for individual differences in judgments of a particularly difficult form of this illusion. The implication of Gardner's (in press) findings concerning Field-Articulation and the Müller-Lyer illusion is that individual differences in personality organization are of importance in determining the magnitude of the illusion.

In view of the gross discrepancies between these and other recent interpretations of this illusion, the present study was designed to provide a test of the selective attention hypothesis under more carefully controlled conditions and with a larger sample of Ss than have characterized previous studies. Although similar in some ways to Benussi's (1904, 1912) early demonstrations that Ss show greater illusion when employing a "whole-perceiving" attitude than when employing a "part-isolating" attitude, the present study also differed from his in that it was based on the assumption that the magnitude of the illusion can be reduced by very brief and limited instructions to concentrate on the horizontal line.

<sup>1</sup> This investigation was supported by research grant M-2454, from the National Institute of Mental Health, Public Health Service.

## METHOD

*Subjects*

Ss were 42 female business college students, university students, and housewives whose age range was 16 to 43, mean age 23.8.

*Apparatus and Procedure*

*Müller-Lyer Illusion.* The illusion figure was constructed on a 28x44-inch black cardboard background with strips of white plastic tape  $2\frac{1}{2}$  mm. wide covered with white tempera paint. Each arrowhead consisted of two lines, each 70 mm. long, which met at a 60-degree angle on the horizontal line. The standard line on the left side of the figure was 325 mm. long. S adjusted the variable line by turning a control knob connected to a selsyn arrangement which moved a covering strip of black cardboard over the variable line. The room was completely dark except for a circular patch of light which illuminated the illusion figure. This light was bright enough to allow the white lines of the figure to be clearly visible but dim enough to obscure the covering strip and other details of the apparatus. Nothing was visible in the room except the white illusion figure. S's adjustment was read from a scale on the back of the apparatus. S was seated in front of the apparatus at a distance of ten feet and dark adapted for five minutes, after which E gave the following instructions:

"I want you to make the distance between the points of these two angles exactly the same as the distance between the points of these two angles (E points with his pen light to indicate the distances to be judged). You can adjust the distance on the right by turning the control knob at your right hand. Be sure you judge the distance from here to here and from here to here (E points again). Start here, be as accurate as you can and tell me when the two distances look exactly equal. Then close your eyes."

After each trial, E reminded S to keep her eyes closed. One ascending and one descending trial were given, from starting points of -200 mm. and +100 mm. respectively.

Half the Ss were arbitrarily assigned to the Rest group, half to the Special Instruction group, so that the two groups were matched in age and in illusion effect on the first two trials. Ss assigned to the Rest group were then asked to keep their eyes closed for one minute, after which they were given another ascending and another descending trial from the starting points used for the first two judgments. Ss assigned to the Special Instruction group were given the following directions before the third and fourth trials:

"Keep your eyes closed for a minute. You remember that there was a horizontal line and some arrowheads or angled lines in the figure. This time I want you to concentrate very hard on the horizontal lines. Try to ignore the arrowheads or angled lines. This is difficult to do, but

I want you to direct your attention only to the horizontal line. Don't squint, just try to limit your attention to the horizontal line. Try not to see the arrowheads. Be as accurate as you can, and tell me when the two distances look exactly equal, just as you did before. Are you clear about what you are to do? All right, open your eyes and go ahead. Remember to concentrate on the horizontal line and ignore the arrowheads or angled lines."

*E* reminded *S* of the special instructions before her fourth judgment.

All *Ss* were then given one ascending and one descending trial with the arrowheads covered, from starting points of  $-120$  mm. and  $+120$  mm. Three vertical lines, each 30 mm. long, were substituted for the arrowheads.

On all trials, *Ss* were allowed as much time as they wished to make their judgments.

## RESULTS

Although the two groups appeared to be closely matched on amount of illusion in the control trials with the arrowheads present, they differed somewhat in judging left and right segments of the horizontal line when the arrowheads were concealed. To control for this difference, an analysis of covariance was done, in which both sets of control judgments were covaried out of the test trial values. The highly significant  $F$  (14.76,  $p < .001$ ) between errors in the test trials by Rest and Special Instruction groups can be attributed to reduction of the illusion as a function of the instruction to concentrate on the horizontal line given to the latter group. This result confirms the hypothesis that selectivity of attention is a determinant of the magnitude of the illusion.

## DISCUSSION

The fact that even brief instructions to concentrate on the relevant horizontal line produced a highly significant reduction in the illusion is compatible with spontaneous remarks made by several *Ss* of the Special Instruction group, who commented that they had already tried to concentrate on the horizontal line in the control trials. These results do not indicate that interpretation of the illusion in terms of "immediate satiation" or other basically non-attentional factors are erroneous. It may be, as Gardner (in press) has suggested, that both kinds of determinants are involved. The present results take their significance from their clear indication that no interpretation of the illusion can be adequate that ignores selectivity of attention.

From Piaget's point of view, the results may indicate that selective attention reduces the centration effects causing the illusion. It should

be noted here, too, that satiation effects are determined in part by the intensity with which attention is directed to the activity leading to satiation (Köhler and Adams, 1958). This fact suggests the hypothesis that Ss most capable of concentration on the relevant horizontal line (if this determinant of the illusion could be assessed independently) show less, or slower, reduction of the illusion in repeated judgments (see Köhler and Fishback, 1950a, 1950b) because they are directing less attention to the angled lines.

The present results also suggest the fruitfulness of a study testing the hypothesis that Ss instructed to concentrate on the relevant line show different retinal induction patterns than Ss viewing the same figure under normal conditions. Confirmation of this hypothesis would have important implications for the means by which selective attention "gates" incoming stimulation.

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## INCLUSION, EXCLUSION, EMPHASIS: SELECTION IN THE HISTORY OF PSYCHOLOGY

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Historians of psychology have reacted to the problem of selecting from the material to be expounded in widely differing ways. Roback, for instance, is severe with himself and his predecessors as well. "The question as to what to introduce and what to eliminate out of the hundreds of books examined" he calls "an ordeal" (Roback, 1952, p. xiii). He illustrates the evils of mis-selection in these words about J. W. Fay's *American Psychology before William James*: "He would cite unnecessarily the opinion of this or that author on his new-found luminary, meanwhile failing to bring out the purport of the latter's main thesis" (Roback, 1952, p. xii). The depth of pessimism which Roback attains is probably unique. He says "no historian can hope to be satisfied with his work, particularly in a first edition" (Roback, 1952, p. xiii).

Select the historian must, nevertheless, if he is to avoid that indiscriminate omniscience under which investigations have been known to collapse. The principles of selection themselves vary in nature, explicitness, and clarity of statement. For instance Baldwin, in a classic short general history, is content to say "a rule of interpretation . . . to guide the selection and govern the estimation of particular facts and theories, is a real *desideratum* in a short sketch like this" (Baldwin, 1913, Vol. 1, p. xiv). Gardner Murphy is more explicit: "Our selection of authors and tendencies must be based, as usual, upon the sole criterion of their significance in relation to our contemporary psychology" (Murphy, 1950, p. 55). These, then, are pronouncements on problems of selection, depending upon a variety of working terms. Some of the latter, like book, author, tendency, fact, theory, relate to the author's unit of study. Others, such as introduction, elimination, criterion, significance, interpretation, estimation, concern the approach to a period or area evaluated by means of these units. Clearly, authors differ on units of treatment, personal involvement or detachment of approach, and level of aspiration towards completeness. These differences may seriously affect the reader's understanding of a period, movement or doctrine, and so his evaluation of an author or work. Accordingly it is the purpose of this article to consider what the historian does, in order to understand better what he ought to do.

### AREA, APPROACH, and ASPIRATION

#### *Two Contrasting Categories*

The first step is to identify those kinds of historical writing where

completeness might most reasonably be expected, and those in which it is for any practical purpose impossible. The first category is represented here by certain review articles from the *Annual Review of Psychology*. The second is made up of selections from historical texts which attempt to cover the whole scope of psychology. Both these categories need some explanation. Review material, it may be said, is contemporaneous within itself, therefore it cannot deal with trend, development, and endurance through time, which are the essence of history. In fact, however, a review article may well cover more than a year or eighteen months. It is true that Brozek is exceptional when he writes: ". . . this reviewer had a fine time perusing a Latin work by Georgius Prochaska . . . ancient (1749-1820) but illustrious *Landsmann* . . ." His point of view may be exceptional also, in its justification of "excursions into history" with the words ". . . a study of the filiation of important, fertile ideas is more than an innocuous pastime" (Brozek, 1958, p. 71). Nevertheless, shorter retrospective surveys, e.g. over the last ten years, are not unknown, and show that in principle the reviewer may well be seen as among other things a short-term historian. For instance, one reviewer counts it among the merits of his predecessor in the same task, that he "provided a good account of the historical development of motivation theory" (Cofer, 1959, p. 173). The historian, for his part, may be a long-term reviewer, especially if, as Roback thinks of Ribot, he "would dwell almost exclusively on the particular men who contributed to the science" (Roback, 1952, p. 4). While this function of summarizing an author's output is to some extent common to all, it remains true that the short text which moves from the Greeks to Gestaltism in a couple of hundred pages has to face an immense number of decisions about including or omitting even major names. It cannot be expected to mention everyone, or even most people. Thus selection is more stringent than in the review, and reasons for it have to be precise, explicit, and cogent.

For convenience in judging what historians do, and for the purpose of comparison with reviewers, three comprehensive texts have been chosen. They are Hulin (1934), Murphy (1952), and Peters (1953). To simplify the comparison further, the treatment of a short period only is dealt with. This begins in 1797, the publication date of the *Encyclopaedia Britannica*, third edition, and ends in 1874, when Wundt and Brentano in their respective ways indicated that psychologies other than British associationism had become important (Wundt, 1874; Brentano, 1874). The relevant material in the respective texts runs to 108 pages in Hulin (1934, p. 67-175), 412 pages in Murphy (1952, pp. 36-448), and 293 pages in Peters (1953, pp. 432-725). The period is selected because of relevance to other historical problems of interest to the writer. Any other would serve, so long as equivalence between the texts is preserved. The principle on which this is done will be explained shortly.



### *Aspiration*

How far, then, do the works in these most-likely and least-likely categories attempt and attain completeness in covering their material? For reviewers, an approximate standard of completeness is furnished by the most relevant volumes of the *Psychological Abstracts*. To the nearest thousand, 9,000 items per annum is a fair figure for these, the numbers for 1958 and 1959 being 6,100 and 11,242 respectively. Now, the *Annual Review* for 1959 cites altogether 2,488 references. These two figures might be combined to suggest 25:90, say 27%, as a ratio of coverage to completeness. But there are ambiguities of reckoning which need to be allowed for. Certain items are cited in more than one reference list in the *Annual Review*. From inspection, 2,000 non-duplicated references appears a reasonable estimate. Moreover, not all review articles cover the same period. Of the 17 articles in the *Annual Review* cited, five cover one year, one 17 months, one two years, one four years, one eleven months, and the rest do not explicitly say. On the whole, 11,000 items is more like the potential material for an article, of which 2,000 instances are covered. On an optimistic view, then, a rough estimate for coverage in relation to completeness, when dealing with recent and readily available material, would be that rather less than 20% of works published receive mention. Even this may be too hopeful, for one reviewer says, "Of some 4,000 articles seen in these journals, only 450 were summarized, and of these only 200 appear here" (Stephens, 1959, p. 109).

The three histories present a very different picture. References noted were limited to explicit citation of works dated between 1797 and 1874. The portions of the historical texts dealt with begin with the opening of the period, and continue to the end of the work to take up any retrospective or specialized references. In 108 pages Hulin cites work within the period 59 times, in 412 pages Gardner Murphy does so 63 times, and Peters has 83 citations in 293 pages. Practically speaking, it is impossible to make an exact statement about the number of works which these writers had to draw upon, but Rand's (1905) figure of 1,639 works cited gives as good an approximation as can be had. All these figures eliminate second mentions of the same work, but include second and subsequent editions. The principles of referencing here followed are more fully handled elsewhere (Cardno, 1961). The estimate of works available for the historian to draw upon is if anything conservative. The proportion of actual to potential citation in these texts, then, is approximately 4% (Hulin and Murphy), or 5% (Peters). Though this figure is nearly the same as that for the proportion actually noticed by Stephens, it is much lower than the proportion he summarized and the review references in general.

Thus here there are two widely differing levels of coverage, even the higher of which is nothing like complete. The reasons for this must now be considered.

### *Approach: the Gordian Knot*

*Retreat, rejection and ruthlessness.* The authors in the first or Annual

Review category do not see the exclusion of material as quite the Gordian Knot that it is for Roback. Some writers, it is true, appear to retreat from their field, but this happens verbally rather than in fact. "No matter how the educational psychologist may define his subject," says one, "he will not be surprised to find his most useful materials, not in this chapter"—but in eight others, all specified (Stephens, 1959, p. 109). Another line is to reject the field as a viable unit, though still handling what conventionally falls within it. Thus personality is held to be "on its way to oblivion" as an "independent, isolated compartment" (Blake & Mouton, 1959, p. 203). The reviewers become even more stringent later. "'Gad, what a mess!' constitutes one appraisal of personality literature," they say (Blake & Mouton, 1959, p. 226). Although different reasons are given at this particular point, these statements suggest that the material is so vaguely delimited or so diverse in kind that it is doubtful whether any coverage would be adequate or not. More generally, the area is said not to be a coherent unit, but the material conventionally supposed to belong to it is dealt with.

The positive expression of this attitude occurs in two ways. The Gordian Knot may be cut by explicit ruthlessness. Thus: "Any selection procedure is bound to be more instead of less arbitrary and unjustifiable. The one used here won't be depended upon beyond stating its purpose, which has two parts. They are "(a) to cover extensively the areas in which the greatest amount of work has been done during the year and (b) to present material outside the major areas in terms of its apparent significance to the field" (Gilchrist, 1959, p. 233). Alternatively, the reviewer may try to estimate what he is excluding and characterize the process by which he is doing it. Thus Cofer (1959) supplements the previous *Annual Reviews* with an examination of 25 periodicals, those in which an American psychologist would "naturally" look, as they are the ones, by and large, which are "standard" (Cofer, 1959, p. 174). Then, since he is dealing with a "pervasive" subject, it "seemed wise" to Cofer to look further. This extends to 39 other journals in allied fields, and proceeds by a process which is interesting for its results at the moment and in its own right later on.

This is "accidental sampling," which leads to the following conclusion: "Although in many instances only one or a few issues rather than the complete file for the period were covered it seems safe to say that these journals yield very little of direct interest" (Cofer, 1959, p. 174). Or again, the reviewer may state *tout court* that he did *not* search certain material. For example, "Of the 3,500 or more educational serials listed by UNESCO . . . only 95 were searched in any systematic fashion" (Stephens, 1959, p. 109). But the reviewer immediately cites summaries which will help to rectify bias and omissions. These reactions to reviewing must now be compared with the historians' handling of their wider but more flexible problems.

*Individuality and impression.* Flexibility is forced upon the historian,



but in diverse ways and for varying reasons. Murphy, for instance, would agree with Roback about the difficulty of selection but suggests a comprehensive rationale for dealing with it. He admits, generally, that "When one considers that the *Psychological Index* carries thousands of titles annually, one may well ask by what right a mere handful of these are mentioned" (Murphy, 1950, p. xiii). He has been influenced by three factors, of which, "perhaps most important of all is my own conception of psychology and my own personal interests" (Murphy, 1950, p. xiii). From quite another angle, Peters supports Murphy, and agrees with his prototype Brett in making the ambit of psychology so wide that no human writer could cover it in its fullness, and also in allowing selection within it. "Patient, passive, presuppositionless enquiry," he says "is a methodological myth" (Peters, 1953, p. 22). And later, "Just as when we walk into a room we notice what is of interest to us, so when we are engaged on an enquiry we collect the type of 'data' or make the kind of observations which are relevant to our interests and previous assumptions" (Peters, 1953, p. 23). Hulin, writing a short, elementary and non-technical account, is concerned that "the student . . . should know something of the origins of current ideas, . . . which theories have stood the test of time, . . . what mistakes have been made" (Hulin, 1934, p. ii). This is a principle of selection, like Murphy's and Peters', but it is a directive and evaluative one. He will try to show, says Hulin, "the gradual change from confusion and self-reference to the cautious, impersonal methods of scientific reflection . . ." (Hulin, 1934, p. 8). The effect of this is summarized at the end of his Chapter 9, "Modern Scientific Psychology", which presents "The first complete psychological systems based on the results of scientific methods. The separate discoveries of the earlier physiologists and anatomists were placed in their systematic setting and thus integrated into comprehensive theories of the mind" (Hulin, 1934, p. 128).

Interest of one kind or another, then, is a basic principle in historical selection. Beyond interest there appears evaluation as already indicated from Hulin. But how explicitly do individual standards of evaluation issue in selection? The historians differ here in their methods but all suggest criteria which have greater objectivity than any personal preference. Murphy's standard includes originality and relevance, whether to the time of writing or to some later period within his history. Thus he says, "I have chosen as best as I could in terms of the importance which attaches to each problem; an elaborate investigation extending a known principle might well be omitted, while a brief and inadequate treatment of a significant new problem might receive attention" (Murphy, 1950, p. xiii). Elsewhere he describes himself as proposing "to throw into relief a few movements whose influence was still strong at the opening of the nineteenth century" (Murphy, 1950, p. xi). More particularly, he explains that he cannot deal with "all the German psychological literature in the age of Herbart," so he selects the "anthropologists" as of greatest contemporary importance (Murphy, 1950, p. 55). This is an approach which *excludes* material when significance,

influence, and importance are weak, in the judgment of the historian, reinforced by his knowledge of the period.

Peters illustrates the contrary approach, by *inclusiveness*, but on grounds of influence again. Why, for example, should much of Brett's philosophical material be retained? "Philosophical enquiries," Peters says in part, "are enormously important in the history of psychology . . . enquiries about man have been influenced by prevailing assumptions, . . . about knowledge and how to obtain it. The welding together of a mass of Brett's material under such headings as 'The Rationalist Tradition' . . . is an attempt to exhibit this influence" (Peters, 1953, p. 29). Hulin is more evaluative, as has been shown, and rather less explicit. He is concerned with influence also, for example his tenth chapter deals with "four specialized fields of study, all of which strongly influence the direction of psychological progress" (Hulin, 1934, p. 159). That this 'progress' is not mere effluxion of time is shown by his formulation of the purpose of the whole work—to give the student "a proper sense of value and a perspective of the present-day opinions in psychology . . ." (Hulin, 1934, p. ii).

The third principle has already been hinted at in Hulin's mention of "perspective." It is orientated to the reader, not the author or the material. When "a movement is represented by many titles," Murphy remarks, "I have preferred to quote one individual's research, making his methods and results clear, rather than to indulge in generalizations which the reader would find it difficult to verify" (Murphy, 1950, p. 13). The problem appears to Peters in a special form, the "abridgement of another man's work." He has not only to decide what is to be omitted, but to devise "a framework for knitting together what is left" (Peters, 1953, p. 10). Inevitably this "creaks," says Peters, but it is essential to the "presentation of such a mass of material," and is combined with "a methodological commentary which provides a unifying theme of interest to modern philosophically minded students of psychology" (Peters, 1953, p. 10).

### *Likeness in Difference*

Superficially, the texts reviewed appear as variable as *reportage* in psychology well could be. Yet they agree in more basic respects than one. Peters is the least pressed, because even when abridged, his work is full and comprehensive. But all writers, both reviewers and historians, confess themselves forced by pressure of relentlessly prolific material to reject, to excise, and to give play to personal preferences. They are driven to depend on impression, which is the ever-present substratum of which notions such as significance, importance, sense of values, are the overt expression. This dependence may be implicit or it may be developed explicitly as by Cofer, who is a model of sophistication in expounding the steps he has taken, and may serve as a paradigm of what takes place.

In the review on motivation referred to, Cofer remarks that guide-

lines are scarce, there has been no general textbook since 1936, motivation is not a category in the *Psychological Abstracts*, the Nebraska symposium is a potential guide but "not designed to serve the needs of taxonomy" (Cofer, 1959, p. 173). Yet "everyone" has some notion of what the term refers to. It is in the light of this "highly general conception of motivation" that he proceeds to search the likely journals (Cofer, 1959, p. 174). To this in a wider sphere answer Peters' concern with framework, methodology, non-duplication of existing histories, and his more deeply seated anxiety about the change in philosophical temper over a generation (Peters, 1953, pp. 9-10). To it correspond Murphy's interest in the importance of each problem, Hulin's insistence on his theme, and Roback's pessimism. All the authorities also agree that there is some process of selection. This varies widely; it may be little more than the 'space does not permit' beloved of commentators less eminent than those cited. It may be the clear exposition of one author rather than the vague generalization about many which is stressed by Murphy. But most suggestively of all, it may be the accidental sampling of Cofer, which needs further exploration.

#### SAMPLING, SELECTION, CHANCE and CHOICE

##### *Accidental Sampling*

Cofer's accidental sampling is typified by the way in which he reports consultation of certain periodicals—"only one or a few issues rather than the complete file." Analogous to this is Murphy's exposition when it depends on one author in a movement. The rationale of both procedures appears to be this: search the literature which is accessible, indicate what is not, and draw non-quantified and guarded inferences to the whole of the field (be it series of a journal or movement in history) from the limited number of examples which are both available and usable. This involves searching, not mere browsing or recollection on the basis of general erudition. But it depends also on impression, on the feeling that such-and-such is "standard," would be "naturally" looked for. (Here Cofer's use of quotation marks is a sophistication in itself). It remains to enquire whether this subjectively based selection can be made more systematic, and if so, whether this could be done by its becoming sampling as known to science, with the greater precision of argument which this would give.

##### *Sampling and Selection.*

Accidental sampling, then, is a sophisticated and explicit instance of a certain kind of approach to the problem of selection. The field of choice is chosen on impression, but once chosen is explicitly delimited. The selection is small, and sub-selections may well vary from one sub-field to another in the proportions they bear to these sub-fields. The proportions are usually known, though they are settled on impression of what is a 'fair thing.' This means that accidental sampling is in fact a kind of literary selection, not scientific sampling at all. Selection, on a standard definition, is "the emergence of certain items, following

the application of a working principle, as belonging to a distinct group" (English and English, 1958, p. 484). The same authority distinguishes sampling from selection, for "all sampling (as contrasted with selection) involves chance" (English and English, 1958, p. 472). It may be that sometimes literary selection (or accidental sampling) may approach block or stratified sampling, especially in the matter of proportions. It remains true, however, that there is a difference in essence between selection on a principle pre-determined by impression (e.g. general reading) and "drawing a sample by chance, i.e. in such a way that every item in the population has an equal and independent chance of being included in the sample" (English and English, 1958, p. 472). It is interesting that both reviewer and historian, so far as has been ascertained, are not given to using sampling as distinct from selection, although reviews are an accepted yardstick for appraising scientific material, and many historians of psychology are also scientists. Yet the present position, where everyday accident, not chance, sets unknown limits to the accuracy of historical judgment, is not a happy one for any writer who wishes to reduce the welter of historical material to some kind of order. Zeigarniks in various senses are likely to abound. Especially is this so if he wishes his procedure to be repeated and his conclusions to be tested.

#### *Population, Item, Unit*

However, the historian of psychology has far to go before he can even begin to sample. First, does he properly know his population? Within the period 1797-1874 the standard bibliography enumerates 1,639 works, as was said earlier. But even this is selective, as is any known bibliography in this field. Rand applied his own principles of selection. "No attempt . . . has been made to compile an absolutely exhaustive bibliography" (Rand, 1905, p. xi). Yet "the endeavour has been to preserve in the amount of material as governed by a work of the present size the relative importance of authors and subjects" (Rand, 1905, pp. xi-xii). If this limitation is accepted, and the population for historical selection is defined as that which a given bibliography contains, what is the effect of each item being given an equal and independent chance of appearing?

Suppose that two random samples are taken from a bibliography of the period mentioned. In the first are James Mill's *Analysis* (1869), Bain's *The Senses and the Intellect* (1855), and J. D. Morell's *Mental Philosophy* (1862). The second consists of Hamilton's *Lectures* (1859), Carpenter's *Mental Physiology* (1874), and McCosh's *Intuitions of the Mind* (1860). If these were the samples, they would give differing views of the psychology of the period, one predominantly associationistic, the other dominated by free-will, to mention only one of many contrasts. The examples given are by no means as mutually contradictory (or plain confusing) as others which could easily arise. In fact, the samples would be much larger, but it would be practically impossible to ensure, on this basis, that sampling would yield views of

psychology less biased and better organized than impressionistic selection would. In other words, the work or book as such is an item which is not a unit in any sense which sampling can make use of. This is a situation which was indicated by the terms mentioned earlier. Author, tendency, fact, theory, problem, are all efforts, more or less conscious, to find a working unit for the history of psychology. The collateral use of terms like interpretation, significance, indicate that historians have little expectation of proceeding beyond qualitative evaluation. Yet the "ordeal" of Roback (who seems to regard books and authors as units) (1952, p. xiii) suggests that something more precise is wanted. Nothing has been found here to show that sampling would be inadequate in this as in anything else, given a unit which is a unit, i.e. an item which can be "treated as a uniform whole" (English and English, 1958, p. 571). The establishment of such a unit is in itself material for a further study, leaving aside the questions whether varying units are needed for different purposes, and whether in some fields quantification may still prove impossible.

### SUGGESTION

It would be excessive to put forward anything approaching the conclusion that clinches an experimental enquiry, on the basis of the foregoing material, which is a small selection from a huge, varied, and relatively unordered field. This much may be suggested, nevertheless. The reviewer is guided by current bibliographical lists and the efforts of his predecessors in the field, on the one hand. On the other, he is influenced by his impressions of what are the 'natural' sources to go to in the circumstances, by what the 'ordinary reader' would regard as a fair definition of the concept he has been asked to deal with. The historian proper is aware of bibliographical aids, of previous works where background information is to be had (e.g. Peters, 1953, pp. 733-736), and of historiography in general. In both reviews and texts, however, there is repeated mention of personal preference, enforced eclecticism in choice of material, and evaluation which is not merely individual but not impersonally scientific either. Taken in conjunction with what has been said about impression, this suggests that there is a small 'hard core' of major works which are 'musts' for any historical study, certain famous names which the historian cannot fail to recall as he makes his first preliminary notes, perhaps from memory. Around this lies a fluctuating penumbra of authors, respectable though not great, who may or may not be noticed according to preference, pressure of space, and so on.

The definitive evaluation of a period or an author, then, will be reached only through a number of laborious clarifications of procedure. These would approach the central problem of choice by various paths. The more literary of these would be the establishment of minimum lists for periods on a more certain basis than now exists. The more scientific would be the delimitation of those genuine historical units

which have been mentioned. Strictly speaking, the problem of units ought to be tackled first. But given the variety of backgrounds, interests, and skills which historians of psychology possess, it may be more fruitful in fact to allow the literary approach to proceed concurrently, if prematurely, alongside the scientific one, and to allow the two trends to cross-fertilize each other.

### SUMMARY

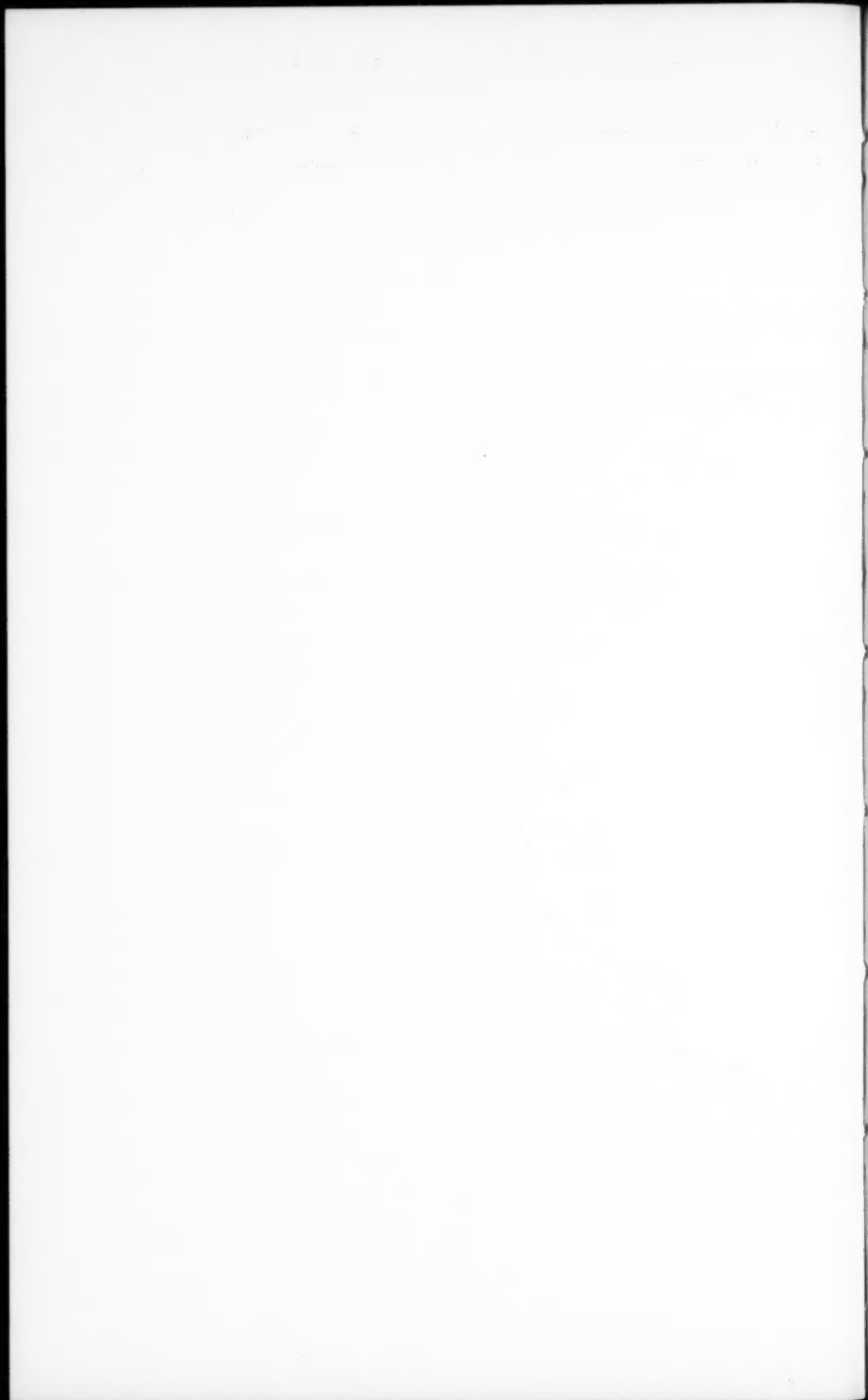
The pessimism, aspirations, and approaches of certain historical texts are outlined. The ratio of treatment to available material is estimated for the most hopeful and least hopeful fields (short-term reviews and comprehensive brief texts). Principles of selection as suggested by the terms used, are outlined. Evaluation is identified as being more objective than personal interest, but less so than scientific sampling, through the presence of accident, not chance. Sampling proper, it is suggested, will not be feasible while accident remains a constituent of selection, and while the unit of treatment (e.g. author, work) remains literary, not scientific. The establishment of genuine units for historical studies is suggested in principle as the next step towards scientific status for this branch of psychology, though in view of the varying skills of historians, concurrent pursuit of literary (e.g. bibliographical) and scientific studies may in fact prove more rapidly fruitful.

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## BEHAVIOR VARIABILITY AND REACTIVE INHIBITION IN HUMAN STYLUS MAZE BEHAVIOR<sup>1</sup>

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One of the main explanatory principles which attempts to interpret variability of behavior is that of Hull's "reactive inhibition." Lepley and Rice (1952) have extended this concept of reactive inhibition to account for variability of maze behavior in the paramecium. In their reformulation of the Hullian postulate, Lepley and Rice state:

"When any reaction occurs, the probability of its later occurrence is reduced. The probability of its later occurrence increases with the continuing succession of following, dissimilar reactions. It is proposed that the probability of later occurrence would be reduced progressively with continued repetition; and that the degree of reduction would be proportional to the degree of repetitiousness. It is further proposed that this reduction in the probability of occurrence for a particular reaction would enhance the probability of occurrence for other reactions in the repertoire."

To test the operation of this principle with the paramecium, Lepley and Rice formulated the following three hypotheses:

"1. When an animal approaches a T choice point along a path without turns, the probability of its turning right or left will approximate 50:50.

2. When an animal approaches a T choice point along a path requiring one preceding right (left) turn, the probability of its turning in the opposite direction will be enhanced.

3. When an animal approaches a T choice point along a path requiring *two* preceding right (left) turns, the probability of its turning in the opposite direction will be further enhanced."

The results of their experiment supported only the first two of these hypotheses. The third hypothesis was not supported, since a decrease in opposite turning was found for the third condition. To account for this, Lepley and Rice suggested that the distribution of the animal's activities, in time, between the turns may permit a partial dissipation of inhibitory effects with consequent decrease in opposite-turning behavior.

Grosslight and Ticknor (1953) retested the three Lepley and Rice

<sup>1</sup>This study was done while both authors were at the University of Wichita.

hypotheses using meal worms. In addition, they formulated a fourth hypothesis as follows:

"4. When an animal approaches a T choice point along a path requiring one preceding right (left) turn, the probability of turning in the opposite direction is related to the distance between the first turn and the choice point; the longer the distance, the closer the turning behavior will approach a 50:50 expectation."

This fourth condition provided a test of the Lepley and Rice suggestion that reactive inhibition dissipates with time (distance). The data of the Grosslight and Ticknor experiment supported all four hypotheses Rice (1953), using paramecia, later verified the fourth hypothesis added by Grosslight and Ticknor.

Rice and Lawless (1957) found that 420 flatworms (*Planaria dorotocephala*) did not perform as would be predicted by the reactive inhibition principle. In no case did the animals turn in the predicted direction more often than chance would allow.

Thus, in previous experiments with paramecia, mealworms, and flatworms, the reactive inhibition principle successfully predicted behavior in a simple T maze preceded by one or two forced turns in the former two cases but not in the latter. Thompson (1949) appeared to demonstrate similar turning behavior in humans predictable by reactive inhibition.<sup>2</sup>

It was proposed in the present study to use the planaria maze, which was suitable for use as a human stylus maze with human subjects and to investigate the possible operation of reactive inhibition principles in the stylus maze behavior of the human. This was a further attempt to test the three Lepley and Rice hypotheses and the fourth Grosslight and Ticknor hypothesis.

## METHOD

### Subjects

The Ss were 300 undergraduates at the University of Wichita in May 1957, of whom 213 were male and 87 were female. All were students in a beginning psychology class.

### Apparatus

The multisectioned stylus maze used for this experiment was the same one used for the planaria by Rice and Lawless (1957).

Starting areas, connecting pathways, and goals were cut through a sheet of Plexiglas 3 mm. thick. This piece was then cemented to a Plexiglas base, this providing a maze of uniform depth. By employing

<sup>2</sup>It should be noted that Buel and Ballachey (1934) and Zeaman and Angell (1953) have observed behavior that could be due to  $I_R$  in albino rats and Thompson (1949) has found  $I_R$  in humans while Schneirla (1929) observed similar behavior in ants. On the other hand, Arbit and McLean (1961) did not find  $I_R$  in the earthworm, *Lumbricus terrestris*.

a system of rubber "blocks," various sections of this maze were made to provide the 15 maze variations used in this experiment.

With this system of blocks, three distance variations for each of five major experimental conditions were formed as follows:

1. T: control mazes with pathways of 4, 8, and 20 cm., respectively, with no turns between the starting and choice point.
2. R-T: experimental mazes with pathways of 2, 6, and 18 cm., respectively, between one forced right turn and choice point.
3. L-T: experimental mazes with pathways of 2, 6, and 18 cm., respectively, between one forced left turn and choice point.
4. R-R-T: experimental mazes with pathways of 2, 6, and 18 cm., respectively, between two forced right turns and choice point.
5. L-L-T: experimental mazes with pathways of 2, 6, and 18 cm., respectively between two forced left turns and choice point.

It may be noted that the length of each of the T control maze pathways exceeds the length of the forced turn to choice-point pathway of its comparable experimental mazes by 2 cm. The maze was constructed so that the length of the starting area to choice-point pathway of each of the T control mazes was equal to the length of the starting area to choice-point pathway of its comparable experimental mazes, instead of equal to the forced turn to choice-point pathway. It was felt that a pathway in the T control group equal to the total distance traversed in the comparable experimental maze pathway would furnish the most adequate basis for comparison.

#### *Procedure*

Each S was blindfolded and a stylus was placed in his or her preferred hand while S was standing at a counter thirty inches high. The stylus was placed in the appropriate starting area in the maze and the following instructions were given.

"Here is the start. Slide the stylus along the groove until you reach the goal. Do not retrace your path and do not lift the stylus from the groove. Try to reach the goal as soon as possible. Ready—begin."

A record was kept of the direction taken at the choice point (the midpoint between A-B, C-D, E-F) and any Ss who retraced their pathway or who jumped the stylus out of a groove were eliminated from the data. Each S was given only one trial in any one of the fifteen conditions. The entire procedure for a single S took less than a minute.

Chi square was used to test the significance of the difference of turns in a given direction from chance. A chi square value of 3.841 was necessary for a deviation to be significant at the .05 level of confidence and a value of 6.635 was required at the .01 level.

## RESULTS

The results may be seen in Table 1. There were 37 records eliminated because of failure to comply with instructions from a total original *N* of 337, leaving 300 *S*'s.

TABLE 1  
TURNING BEHAVIOR AT A CHOICE POINT IN HUMAN STYLUS  
MAZE AS A FUNCTION OF DISTANCE

		Right	Left	Chi Square
T (control)	4 cm.	10	10	0
	8 cm.	10	10	0
	20 cm.	8	12	.20
R - T	2 cm.	4	16 <sup>a</sup>	7.20 <sup>**</sup>
	6 cm.	3	17 <sup>a</sup>	19.60 <sup>**</sup>
	18 cm.	3	17 <sup>a</sup>	19.60
L - T	2 cm.	13 <sup>a</sup>	7	1.80
	6 cm.	9 <sup>a</sup>	11	.20
	18 cm.	12 <sup>a</sup>	8	.80
R - R - T	2 cm.	4	16 <sup>a</sup>	7.20 <sup>**</sup>
	6 cm.	8	12 <sup>a</sup>	.80
	18 cm.	8	12 <sup>a</sup>	.80
L - L - T	2 cm.	16 <sup>a</sup>	4	7.20 <sup>**</sup>
	6 cm.	9 <sup>a</sup>	11	.20
	18 cm.	9 <sup>a</sup>	11	.20

<sup>a</sup> indicates predicted direction.

<sup>\*\*</sup> equals a difference significant at the .01 level of confidence (6.635 required)

It can be seen that the number of right and left turns in each of the three T control maze conditions was not significantly different from chance which supports hypothesis 1. It may also be noted that with a single right forced turn and at all distances, the turns in the predicted direction exceeded chance expectancy at more than the .01 level of confidence while in the similar conditions following one left turn, no turns exceeded chance expectations (although the difference was in the predicted direction in the shortest and the longest distance conditions).

In the two forced turn conditions, the results followed the predictions of the reactive inhibition principle rather well. In the shortest distance (2 cm.) turns at the choice point in the predicted direction exceeded chance expectancy at the .01 level, but at the longer distances (6 cm. and 18 cm.) turning was not significantly different from chance.

However, it can be seen in Table 2 that if the direction of the forced turn is disregarded, turns are in the predicted direction in excess of the .01 level of chance in the shortest or 2 cm. condition in both the

TABLE 2  
PREDICTED AND NON-PREDICTED TURNING BEHAVIOR IN  
A HUMAN STYLUS MAZE AS A FUNCTION OF DISTANCE

One forced turn	Predicted	Non-Predicted	Chi Square
2 cm.	29	11	8.10**
6 cm.	26	14	3.60
18 cm.	29	11	8.10**
Two forced turns			
2 cm.	32	8	14.40**
6 cm.	21	19	0.10
18 cm.	21	19	0.10

\*\* equals a difference significant at the .01 level of confidence (6.635 required)

one and two forced turns conditions. It is also to be noted that turning in the predicted direction is increased in the two-forced turn condition noticeably over that of the one turn condition (32 to 8 as opposed to 29 to 11).

The odd result is that with one forced turn, as many turned in the predicted direction at the 18 cm. distance as did in the 2 cm. distance. This is contrary to the prediction of hypothesis 4.

### DISCUSSION

In general, the four hypotheses presented are supported by the present evidence, especially in the light of the fourth or Grosslight and Ticknor hypothesis that the greater the distance between the forced turn and the choice, the less likely turns in the predicted direction would appear. In the control conditions with no forced turns, there was no difference from chance at the choice point, in the shortest one forced turn condition, turns were in the predicted direction better than chance would allow. In the shortest two forced turn condition, turns in the predicted direction were even more noteworthy, all of which would follow reactive inhibition predictions. The most outstanding exception to these predictions was strong turning in the predicted direction in the longest one forced turn condition which is not accounted for by any of the hypotheses and the other is the fact that in the left forced turn condition, 20 Ss did not turn in the predicted direction.

These results suggest that variables other than reactive inhibition

may be operating even if  $I_R$  is present in humans as suggested by Thompson (1949) and by the present results.

### SUMMARY

In previous experiments with small animals, paramecia and mealworms followed the predictions of  $I_R$  in their maze behavior while flatworms did not. In the present experiment, 300 undergraduate psychology students in general did behave largely as would be predicted by  $I_R$  in a multisectional stylus maze. When the distance between a forced turn and a choice point was short (2 cm.), students turned in the direction opposite to that of the forced turn more often than chance would predict. In the longer distances, they did not turn in the predicted direction significantly more often than in the non-predicted direction with the single exception of the longest (18 cm.) distance in the one forced turn condition.

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## PhDs IN PSYCHOLOGY WHO FUNCTIONED AS CLINICAL PSYCHOLOGISTS BETWEEN 1896 AND 1910

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The information for this brief communication was obtained from first-hand questionnaire returns in 1913 (Wallin, 1914); from extensive correspondence and examination of college and university catalogues; from first-hand observation of the programs in many institutions and school systems between 1910 and 1912; and from a recent communication (Mensch, 1960).

The first-hand visitations and inspections during this period included institutions for mental deficient in Vineland, N.J., Elwyn, Pa., Waverley, Mass., Columbus, Ohio (where I gave the Binet for the first time in that institution in 1911), and Owings Mills, Md.; institutions for epileptics in Palmer, Mass. and Skillman, N.J. (where I established in 1910 the first laboratory of clinical psychology in such an institution in the world); institutions for psychotics in Trenton, N.J., Clarinda, Iowa (where I administered the Binet presumably for the first time in such an institution), Chicago, Ill., the Government hospital in the District of Columbia, and the Sheppard and Enoch Pratt Hospital near Baltimore, Md. The public school installations visited included the special education setups in Chicago, including the Department of Child Study and the residential schools for delinquents (as well as the detention home and the juvenile psychopathic institute), and the special classes in Cleveland, Columbus, Washington, Baltimore, Philadelphia, New York, and Pittsburgh (Wallin, 1946, 1953, 1955, 1957, 1958, 1961).

During these early years the mental examiners in the public schools were, essentially, amateurs. Investigations showed that about three-fourths of them were teachers, principals or supervisors (Wallin, 1914, 394 f. Table III), most of whom were merely normal school graduates who had pursued a course in Binet testing and perhaps one or two other professional courses in a summer school. They were often referred to as "Binet testers," not necessarily derisively, because they were dependent almost exclusively upon the Binet scale for diagnostic guidance.

The Ph.Ds in psychology who conducted psychological clinics or the equivalent prior to 1911 include the following 14 men and two women of whom only two were connected with public school systems:

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Lightner Witmer (Ph.D., University of Leipzig, 1892), founder of the first psychological clinic in the University of Pennsylvania, 1896, and originator of the term. He died in 1956 at 89.

William O. Krohn (Ph.D., Yale University, 1889), psychologist at the Eastern Hospital for the Insane at Kankakee, Ill., 1897-1899. He died in 1927 at 59 (Mensch, 1960; Wallin, 1961 A).

Arthur A. T. Wylie (Ph.D., College of Wooster, 1894), post doctoral work in psychology at Clark University, 1902) who conducted anthropometric and psychological studies on part time in the Minnesota School for the Feeble-minded for several years beginning in 1898, a research rather than a clinical position.

Daniel P. MacMillan (Ph.D., University of Chicago, 1900), assistant in the Department of Child Study and Pedagogic Investigation in the Chicago Public Schools, 1900 (director 1902-1935). He died in 1947 at the age of 77.

Shepard I. Franz (Ph.D., Columbia University, 1899), pathological psychologist in the McLean Hospital, 1904, and psychologist and later scientific director in St. Elizabeth Hospital in Washington, 1907-1924, both purely research positions when the writer inspected the work in 1911. He died in 1933 at 59.

Frank G. Bruner (Ph.D., Columbia University, 1905), psychologist in the Chicago Department of Child Study and, apparently, director of special schools, 1905. Apparently still alive at 87.

Clara H. Town (Ph.D., University of Pennsylvania), resident psychologist, Friends Asylum for the Insane, 1905-1910. Still alive at 86.

Henry H. Goddard (Ph.D., Clark University, 1899), director of research in the Training School at Vineland, 1906-1918, a combined research and clinical position. He died in 1957 at 90.

Frederick L. Wells (Ph.D., Columbia University, 1906), assistant in pathological psychology, McLean Hospital, 1907, apparently a purely research position when visited by the writer; still alive at 77; recipient of the first award from the clinical division of the A.P.A.

Jacob D. Heilman (Ph.D., University of Pennsylvania, 1908), director psychological clinic in State Teacher's College, Greeley, Colorado, 1908; apparently still alive at 86.

Stevenson Smith (Ph.D., University of Pennsylvania, 1909), director of psychological clinic, University of Washington, 1909; died in 1950, age 67.

Edmund B. Huey (Ph.D., Clark University, 1899), director of the psychological laboratory in the Lincoln State School in Illinois, 1909; died in 1913 at 43.

Grace Fernald (Ph.D., University of Chicago, 1907), psychologist in the Juvenile Psychopathic Institute in Chicago, 1909; died in 1950 at the age of 70.



James B. Miner (Ph. D., Columbia University, 1903) director of Free Clinic in Mental Development, University of Minnesota, 1909; died in 1943, age 69.

J. E. Wallace Wallin (Ph.D., Yale University, 1901), director of laboratory of clinical psychology, New Jersey State Village for Epileptics, 1910 (prior counseling with backward children, beginning in 1907, without use of standardized tests); research dominantly, but also clinical.

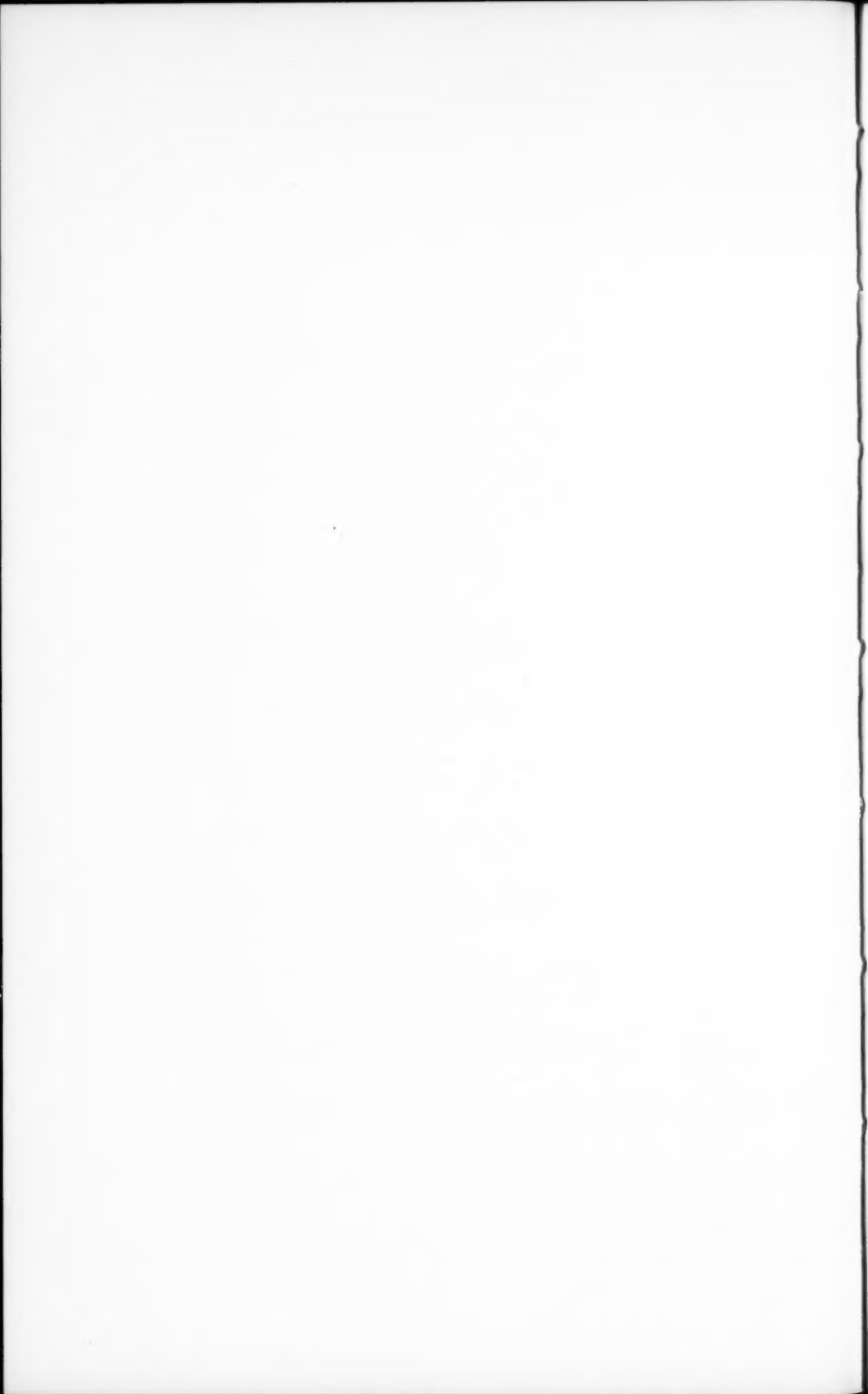
Frederick Kuhlmann (Ph.D., Clark University, 1903), director of psychological research, Minnesota School for the Feeble-minded at Faribault, 1910; research and clinical; died in 1941 at 65.

The universities that conferred the Ph.D.'s on these pioneers were in the order of frequency, Columbia (four), Clark (three), Pennsylvania (three), Yale (two), Chicago (two), Leipzig (one), and College of Wooster (one).

According to report psychological work was done during this early period in the Massachusetts General Hospital (by L. E. Emerson, Ph.D.) but no exact information is available to the writer. A Child Study Department was established by the Rochester public schools in 1907, but the person in charge (Grace Boehme, M.A.) did not hold the Ph.D.

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## SENSORY DEPRIVATION AND SOME THERAPEUTIC CONSIDERATIONS

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It has been noted that some of the behavioral phenomena associated with emotional disorders can be produced in normal people undergoing sensory deprivation. For instance, there is evidence of irritability, childishness, rigidity, anxiety, bizarre fantasies, flickering delusions, transient hallucinatory experiences, and motivational deficits (Hebb, 1958; Wheaton, 1959). On the basis of these similarities, recent efforts have been made to conceptualize abnormal behavior in terms of concepts derived from sensory deprivation studies (Goldfried, 1960; Robertson, 1961; Rosenzweig, 1960).

Before discussing the therapeutic implications of sensory deprivation, it is necessary to summarize a theoretical position presented in a previous article (Robertson, 1961). First, an individual's response to sensory deprivation can be described in terms of preoccupation and suggestibility. Second, using these two concepts, abnormal behavior can be considered a deprivation phenomenon.

For example, a partial type of deprivation would occur when a severe problem distracted the person from much of what normally stimulated him, and he would then become more and more preoccupied with the problem to the exclusion of everything else. His field of awareness would tend more and more to be limited to those external and internal stimuli related to the object of his preoccupation. Or, due to the nature of a particular emotional problem, the individual might be motivated to withdraw from certain types of anxiety-producing stimuli. If the anxiety were sufficiently great and/or the stimuli were sufficiently general or common, the avoidance could be easily extended, so that the individual would in a sense withdraw from or avoid most of the stimuli with which he ordinarily comes in contact. A state of preoccupation would then follow in which the individual would focus on a small circumscribed segment of external stimulation and the pattern of ideational stimuli elicited by this. In either case, external stimulation in addition to having an activating function also has a suppressing or inhibiting effect on some ideational stimuli, and with the loss of external stimulation this suppression effect would be weakened correspondingly and the person would become aware of hitherto unconscious thoughts and feelings. Furthermore, the same restriction of stimuli that would produce the preoccupation would also create suggestibility by magnifying the little that remained in awareness and

by isolating it from reality considerations. The effect of this would be manifested in the individual's tendency to react too freely and uncritically to the residual sensory input, e.g., misperceptions, loss of perspective, uncritical acceptance of many associative reactions, the tendency to project some normally unconscious thoughts and feelings.

Both the range and the intensity of the behavioral deviations would be a function both of individual personality variables and the degree of restriction and duration of the deprivation taste (Hebb, 1958; Solomon, 1957; Wheaton, 1959).

### THERAPEUTIC CONSIDERATIONS

The major therapeutic implication of SD is the very obvious one of reinstating the normal level and range of external and internal stimulation. For some individuals this would mean increasing the total amount of stimulation, while for others it would mean increasing certain important classes or types of stimulation, or perhaps the introduction of variation so as to eliminate monotony and satiation effects. In connection with this point, some of the therapeutic benefit of institutionalization may be lost by a kind of monotony or satiation effect that develops from the unvarying pattern of institutional management. In any case, a therapeutic program for a particular individual would be based on the total amount as well as the pattern of deprivation, i.e., the degree of deprivation within various areas of customary or normal stimulation.

Generally, an increase in stimulation would be therapeutic insofar as it would be instrumental in dispelling the individual's preoccupation by making him alert to his immediate environment. Preoccupation would also be diminished since the external stimulation by preempting his attention would have a suppressing effect on some of the internally produced associations. Suggestibility would be decreased, because with the increased stimulation that which is the source of the suggestibility and preoccupation would no longer loom so large. Furthermore, its meaning and significance would be viewed against a larger background or context of stimulation. A similar effect can be found in the mild depression and work inhibition that normal people experience periodically. If a person forces himself to work, the increased stimulation resulting from the work activity quickly suppresses many of the ideational stimuli and before long the depression and feeling of monotony all but disappear. Furthermore, the idea causing the depression and work inhibition is no longer as compelling because an increasing range and intensity of stimulation is being superimposed on it.

Part of the therapeutic value of physical shock therapies as well as the so-called "rock em sock em" type of psychotherapy may be the sudden jarring increase in stimulation and the temporary dissipation of the effects of preoccupation and suggestibility. If so, then it is not a question of finding better therapeutic techniques to eliminate de-

privation effects but rather how to implement present techniques so that their initial therapeutic effect can be more lasting.

One of the critical problems in understanding and treating emotional disorders is the perpetuation and resistance to extinction of the behavior that brings obvious discomfort and distress to the person. This problem has been previously discussed in terms of some of the behavioral sequelae of the SD state (Robertson, 1961). That is, a deprivation state at first may bring some relief or relaxing of tensions, but as it continues the person becomes sensitized to the loss of certain types of stimulation. But the same deprivation that creates the sensitization and increases the desire to regain such stimulation also weakens the capacity to act on the desire to carry out the wish for such contact. As the deprivation continues, the desire would increase, but the drive to satisfy the desire would decrease. Covert methods of stimulation (symptoms) would develop which would represent both a partial satisfaction of the increasing desire and also the limited capacity of the individual to follow through on the desire. In other words, the abnormal desire or wish for withdrawal from certain stimuli that initiated the deprivation process would exist no longer, and it would be the return of a normal desire or wish of strong intensity combined with a motivational deficit that would perpetuate the maladaptive reaction in the form of covert methods of stimulation. Therefore, from a therapeutic standpoint it would seem advisable to focus on this positive wish or desire rather than the self-defeating one that is no longer operating. By focussing on this positive desire or wish, the therapist is then able to help him give up the distorted expressions of it as manifested in some of the symptoms in favor of a more natural and complete expression of the desire.

Some mention should be made of the potential therapeutic effect that SD may have. Some people in their introspective accounts of the deprivation experiences report a greater sense of inner harmony and integration as an after effect (Lilly, 1956). Even among normal people, perhaps a periodic self-imposed SD state, if used at appropriate times, may be desirable insofar as it enables the individual to experience new dimensions of self-awareness and to explore his deeper feelings and values.

Even more pertinent are the reports of SD being used as a therapeutic method with selected types of hospitalized psychotic patients. (Adams, et al, 1960; Azima & Cramer, 1956) An explanation of the moderate success reported by these investigators would probably touch on one or more of the following points. First, some essentially normal people have been observed to recover from great stress by withdrawing into themselves for a time in order to stabilize their feelings and thoughts and work out a new perspective on their life. Second, one is reminded of the occasional discussion concerning the "wisdom of the unconscious." SD, insofar as it may produce a general suggestibility and loosening of the cognitive processes, would provide a means of ac-

cess to the positive and constructive features of the unconscious. Third, the effectiveness of negative practice in extinguishing habits may have some bearing. (Dunlap, 1928). That is, the effect of giving withdrawn individuals more deprivation than they actually desire minimizes the pleasant and maximizes the unpleasant aspects, thus creating a desire to avail themselves of stimulation. Finally, sensory deprivation may accomplish therapeutically what the physical shock therapies seem to do. First, the deprivation experience may jar the individual out of some over-used thought patterns, with the result that contact and communication with the person would be facilitated. Second, insofar as the deprivation may produce a certain degree of suggestibility, the individual would become more responsive and receptive to specific therapeutic suggestions and advice.

One final point concerns the preventive aspect of SD. Conceivably, it might be worthwhile to consider arranging for limited or graded amounts of SD experiences in order to build up a tolerance for or immunity against some of the serious uncontrolled effects of prolonged deprivation. Some experimentation along this line is undoubtedly being done in the armed forces, particularly in the investigation of space travel.

#### SUMMARY AND CONCLUSION

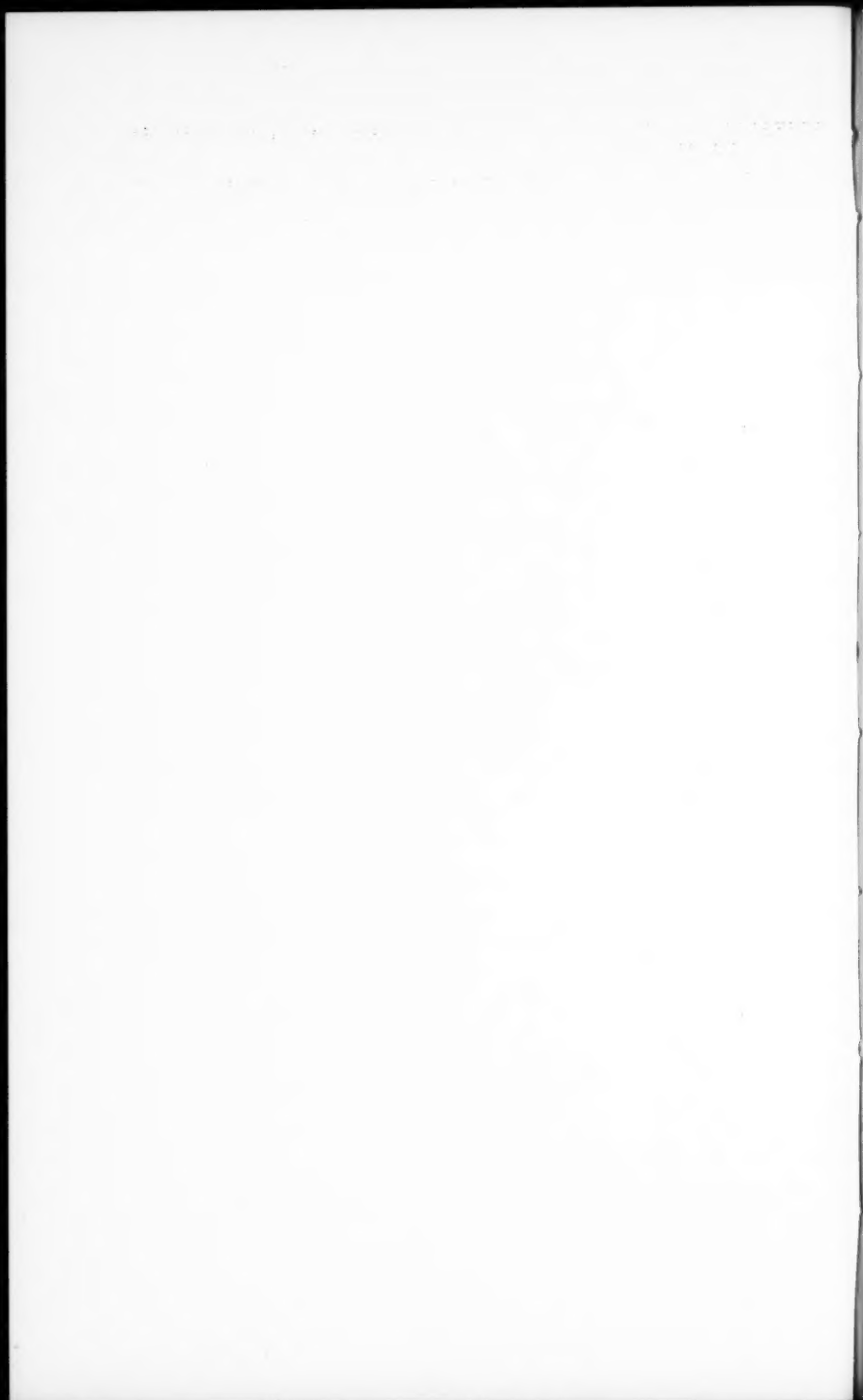
Within the framework of mental health, two theoretical views of sensory deprivation are needed. First, sensory deprivation is identified as an etiological factor of considerable importance in the development of emotional disorders. Consequently, the treatment and prevention of emotional disorders should be based on a judicious appraisal of both the quantitative and qualitative aspects of deprivation.

Second, therapeutic effects of sensory deprivation have been noted. Therefore, the author takes the position that through a controlled and systematic application of deprivation, a more effective utilization of its role as a therapeutic agent can be made.

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## PERCENTAGE TIMING REINFORCEMENT SCHEDULES<sup>1</sup>

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In a recent study (Bolles, 1961) a new technique for scheduling intermittent reinforcement was described. The reinforcement schedule may be designated a "percentage timing" schedule because the central programming device was a percentage timer, i.e., a recycling clock used to set up reinforcement during a given percentage of its cycle. For example, with a 15 sec. cycle timer set at 20%, responses are cyclically reinforced for 3 sec. and unreinforced for 12 sec. Thus, the resulting schedule gives the animal an opportunity to put itself on a fixed interval schedule provided it can learn the appropriate temporal discriminations. With the failure to make the required temporal discriminations, however, the animal is effectively on a variable ratio schedule since a certain relatively constant proportion of its responses will be reinforced over a period of time. On the other hand, the proportion of time that the timer pays off is not the same as the proportion of responses that is reinforced. It has been reported that when the timer payed off 30% of the time, only 22% of the animal's responses were reinforced (Bolles, 1961). Evidently, this particular relationship, and in general, many of the properties of percentage timing schedules must be established empirically. The present study is concerned with investigating some of the properties of these schedules.

### METHOD

Seven male rats were used; five were hooded, approximately 100 days old, and two were albino, approximately 150 days old. Two of the hooded animals were naive, but the others had had prior experience running in an alley.

The apparatus has been described before (Bolles, 1961); briefly it was a two-bar operant conditioning apparatus (Foringer 1102 M). Each bar was connected through a pulse former, an interlock device (which eliminated irregular responses like stuttering and holding from the protocol), and a percentage timer (Industrial Timer Corp. model PC) to its feeder. Responses on each bar were recorded with a Davis cumulative recorder arranged so that a response on one bar moved the pen, and a response on the other bar advanced the paper. Thus, the resulting curve is not a typical cumulative record but a continuous

<sup>1</sup>This study was supported by research grant M-2798 from the National Institute of Mental Health, USPHS.

record of the responding on both bars. The slope of this curve is determined by, and provides a measure of, the relative rate of responding on the two bars. The Ss were maintained at 80% to 85% of their initial body weight, and were run once a day in 25 min. sessions. Reinforcements were 45 mg. Noyes pellets.

The principal phenomenon investigated was the discriminability of differential pay-off probabilities between the two bars. The bar press response was shaped under continuous reinforcement on one bar in the first session; then S was switched over to continuous reinforcement on the other bar in the second session. On succeeding sessions the pay-off probabilities were brought progressively closer together, up from 0% and down from 100%, until discrimination broke down. Evidence of discrimination was obtained from the tendency of S's differential response rate to follow the differential pay-off probabilities. In the early sessions the pay-off contingencies for the two bars were interchanged between sessions, and later in training the pay-off contingencies were interchanged once or twice within each session.

After investigating the discriminability threshold, Ss were run under different conditions; two Ss were run in repeated sessions in a one-bar situation under 25% pay-off to determine if the temporal pattern of the cycle could be discriminated. The other Ss were run with the pay-offs for the two bars equated but set at different values from 100% to 6¼% to determine the extinction ratio and other behavioral consequences characteristic of a given pay-off. The importance of the *phasing* of the two percentage timers has been already indicated (Bolles, 1961). Throughout the present study the phasing of the two timers was varied randomly every few minutes during the test sessions.

## RESULTS

Let us introduce a notation to describe the pay-off probabilities; let "100:0" designate continuous reinforcement on the left bar and no reinforcement on the right bar. After the initial training under 100:0 and 0:100, one S was run under the following conditions: 90:10, 20:80, 70:30, 20:60-60:20, 50:20-20:50, 15:45-45:15, 20:40-40:20-20:40, 35:25-25:35-33:25, 23:37-37:23. (A dash indicates that the pay off probabilities for the two bars were interchanged during the test session.) The performance for this last day, the eleventh session, and for two more days, also under 23:37, indicated in Figure 1.

Figure 1 indicates that this S could discriminate the 23:37 conditions. That is, its relative rate of responding on the two bars shifted when the pay-off was shifted from 23:37 to 37:23 or vice versa. However, there was no evidence of discrimination under the 25:35 conditions; there was no corresponding change in slope either of the two times that the 25:35 conditions were interchanged. Six of the seven Ss were able to discriminate the 23:37 conditions at least once, but only one S was ever able to discriminate the 25:35 conditions. Thus, a well-

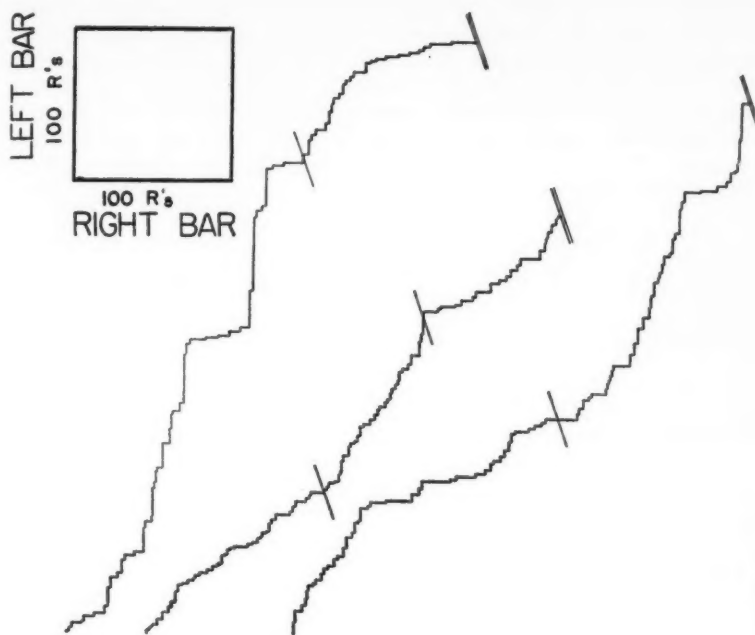


Figure 1 Discrimination of differential pay-off percentages. Cumulative responses on the two bars under conditions 23:37-37:23, 37:23-23:37-37:23, 37:23-23:37.

defined threshold of pay-off discriminability would seem to be obtainable within this narrow range of pay-off probabilities, under the conditions of the present study.

Two Ss were run for 20 sessions under 25:0 conditions to determine the difficulty of the temporal discriminations involved in a 15 sec. percentage timing schedule. As with a fixed interval schedule, the discrimination would be most directly indicated by a progressive reduction in the extinction ratio with continued training, i.e., continued training should lead to a lower ratio of unreinforced to reinforced responses. If Ss distributed their responses randomly in time, then the extinction ratio under 25:0 would be 3.00. But because eating competes with pressing for food during that part of the cycle when the percentage timer is paying off, the observed ratio is somewhat higher than 3.00, viz., 4.50. By the end of 20 sessions, one S had only reduced this figure to 4.15, but the other S averaged a ratio of 2.81 on the last four sessions. Thus, one S at least was able to make the appropriate temporal discrimination.

That this was a temporal discrimination, rather than a response differentiation, e.g., the dropping out of eating during the continuous reinforcement part of the cycle, is indicated by the fact that the reduced ratio resulted from a reduced number of unreinforced responses rather than an increased number of reinforced responses. (It should be noted that these two animals were working in what amounted to a one-bar situation. There was no indication at all that any animal could "solve" the two-bar problem.)

Five Ss were run under conditions where the pay-off probabilities for the two bars were equated but set at different values in different sessions. The conditions were 100:100, 50:50, 25:25, 12½:12½, and 6¼:6¼, and they were presented in an order counterbalanced across Ss. The results are indicated in Table 1.

TABLE 1  
MEDIAN PERFORMANCE FOR 5 Ss AS A FUNCTION OF  
PAY-OFF PERCENTAGE.

Pay-off Percentages	Responses	Reinforce-ments	Extinction ratio	Bar bias <sup>a</sup>	Ave. run length
100:100	195	195	0	.90	62
50:50	447	134	2.33	.77	10.1
25:25	702	127	4.50	.58	5.1
12½:12½	1184	99	11.0	.58	3.2
6¼:6¼	1366	63	20.7	.56	2.5

<sup>a</sup>"Bar bias" is the proportion of total responses on the preferred bar. "Ave. run length" is the average number of consecutive responses on one bar.

It is clear from the last two columns of Table 1 that decreasing the pay-off probability has the effect of improving the linearity of the curve, and also results in an average slope that more nearly approximates unity.

## DISCUSSION

It is interesting to note how closely the performance on a percentage timing schedule is tied to the immediate reinforcement contingencies. The "preference" of one bar over the other, and the tendency to switch bar preference, seems to be controlled by the events within a few cycles, rather than depending upon a large preceding context. The directness of control with this kind of schedule is indicated by the promptness with which S in Figure 1 altered its bar preference when the pay-off probabilities were changed, and also by the promptness with which it adjusted to the prevailing conditions at the start of each

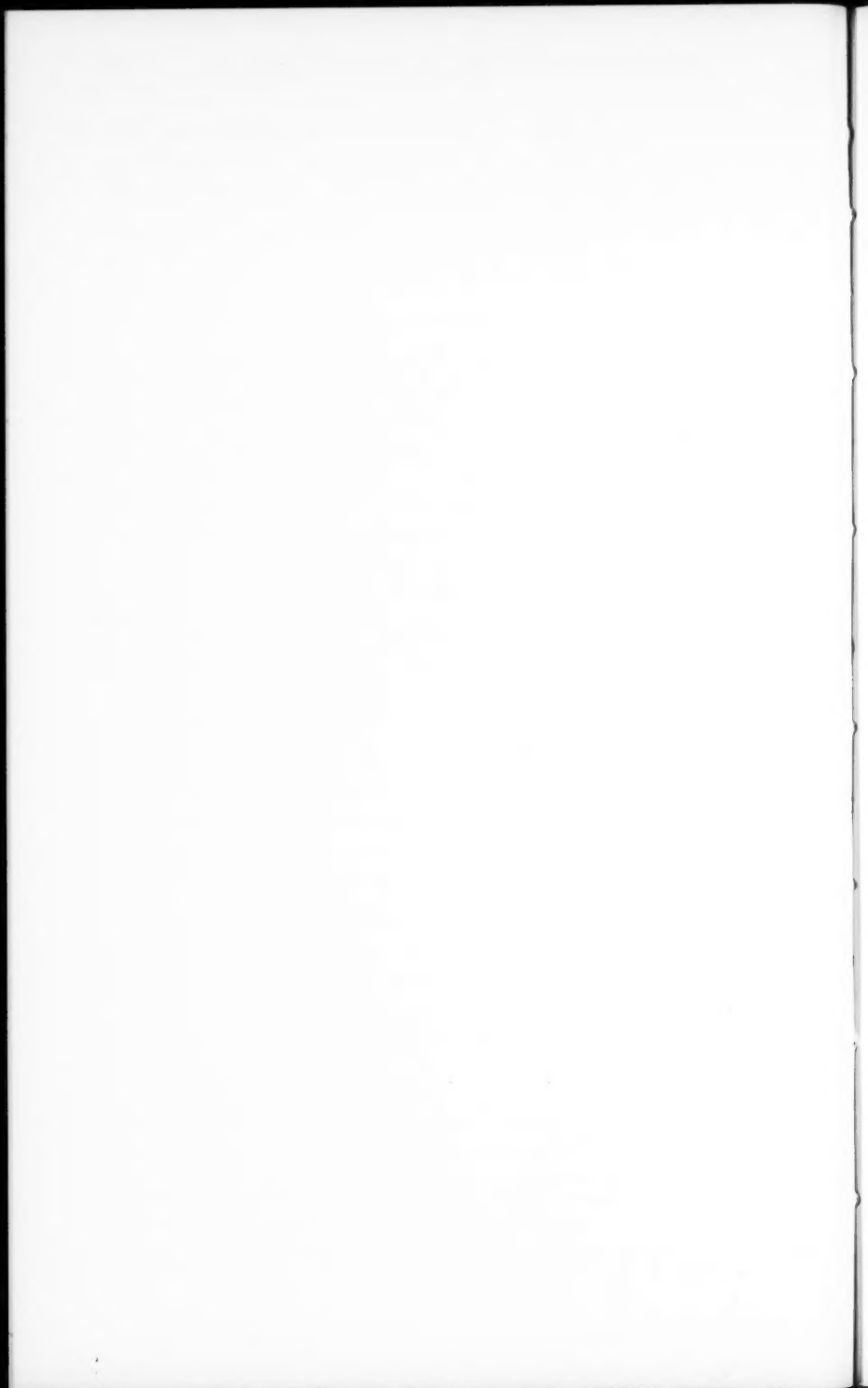
session, particularly the first two. The directness of control in this situation would seem to preclude the interpretation that S "samples its environment"; appropriate pay-off matching behavior is evidenced long before an adequately large stimulus context is presented. The simplicity of the programming apparatus, and the speed with which discrimination was established recommends this type of schedule for developing inter-bar discrimination.

#### SUMMARY

A percentage timing schedule is one where reinforcement is set up during a given percentage of a fixed temporal cycle (15 sec. in the present case). Seven rats were run in a two-bar operant situation to determine the discrimination threshold of differential pay-off probabilities between the two bars. The threshold was found to be just less than the difference between reinforcement 23% and 37% of the time cycle. Other features of percentage timing schedules, particularly the behavioral effects of using different pay-off probabilities, were investigated and are discussed.

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## INTERVENING CONSTRUCTS: THE PROBLEM OF FUNCTIONAL VALIDITY

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The intervening variable paradigm (IVP) was introduced to psychology by E. C. Tolman in 1932. The term "intervening variable" (IV) was intended to signify a functional relation between the observable behavior of the organism and certain predetermining factors, which were identified as hereditary endowment, previous environmental training, present stimuli, and initiating physiological states (Tolman, 1932). Tolman made the point that he considered the IV in his theoretical analysis as having a strictly logical status and as being related to strictly behavioral elements (Tolman, 1932, 1935). Tolman's lead in adapting the IVP to psychological theory was followed by Clark Hull. Hull stressed the necessity of anchoring the IV to antecedent and consequent conditions (1943). While both Tolman and Hull seemed to envision the IV in a functional capacity as a mediating term in the organization of the theory, at the same time they began to associate the IVs in their respective theories with content both of a physiological order and of a psychological order.

The resulting tension between pure functionality and identification in terms of neurophysiological elements or of behavioral data was clarified in the classic paper of MacCorquodale and Meehl (1948). They attempted to distinguish the purely functional status of theoretical concepts from theoretical embodiment in terms of content and structure. The IV was a statement whose terms were reducible to empirical laws, the validity of which was a necessary and sufficient condition for the correctness of statements about it. Quantitative expression of the IV could be obtained without mediating inference by groupings of terms in the empirical laws. The hypothetical construct (HC), on the other hand, was not reducible to the terms of the empirical laws and the validity of those laws was not sufficient to guarantee the truth of the concept, since the concept involved meaning over and above that of its empirical referents.

Although this distinction helped to clarify the terms of the discussion, it did not succeed in eliminating all ambiguity from the use of the terms IV and HC. The IV has been referred to as ultimately neurological and physiological (Hebb, 1951), as a symbolic expression mediating between antecedent and consequent variables (Argyle, 1957; Koch, 1954; Maze, 1954), and even as enjoying existential reference (Rozeboom, 1956). Similarly, the HC has been variously described as a

neurological event or series of events (Krech, 1950) as enjoying existential reference or denoting structural characteristics of the organism (Ginsberg, 1954). Confusion hath made her masterpiece!

As a result of this lack of methodological clarification, psychological theorists have split down the middle on this issue. Reactions vary from the skeptical rejection of HCs and the insistence on the IV as prototype of psychological explanation (Skinner, 1950, 1959) to a rejection of the IV as methodologically confining and an appeal to the more fruitful and less rigid HC (Ellis, 1956; Feigl, 1950s, 1950b; Bergmann, 1953). The latter point of view is usually proposed in terms of a vague feeling that the HC is more congenial to the present level of development of psychology. Other attempts have been made to analyze the types of constructs found in psychology (Meissner, 1960), but the attempted categorizations have succeeded in either reasserting the basic dichotomy outlined by MacCorquodale and Meehl (1948) or in underlining the ambiguities latent in their analysis. That this should be the case is not at all surprising, since the issues involved in this problem are among the most profound that confront modern psychology and certainly are radical in the psychological analysis of almost all behavioral problems.

Since the issues involved in the clarification of the theoretical status of IVs and HCs are so complex and since their implications are wide-ranging, to say the least, I should like to narrow the scope of discussion in this article. There are many specific issues directly related to this whole question—all of them equally important. Moreover, the problem can not be considered adequately without thorough consideration of all of these elements and their interaction. I refer specifically to problems of empirical linkage, definitional status, existential reference, determination of evidential bases, possibility of unambiguous or adequate definition on operational grounds, predictive and explanatory power, ultimate levels of explanation, etc. Some of these issues have been subjected to preliminary discussion elsewhere (Meissner, 1958, 1960). Recognizing this complexity, I would like to focus upon the issue of functional validity.

The predominant drive among theorists has been to schematize theory in terms of the IVP and to regard the goal of scientific theorizing as the reduction of all theoretical formulations to purely functional IVs. The direction of this type of theorizing was set by Tolman and Hull. In his much discussed "Are Theories of Learning Necessary?", Skinner (1950) observed that the most rapid progress toward the understanding of learning may be had by way of research which was not designed to test theories at all. It would be quite sufficient for an acceptable scientific program to relate data to manipulable variables which had been selected through common sense exploration. Skinner's view of the matter has not changed to any noticeable degree (1959). The approach of Skinner, Tolman and Hull has been called a quest for the "empty organism" (Feigl, 1951).

The idealization of the empty organism is an extreme position.



Most theorists who endorse the primacy of the IV also recognize the validity of the distinction proposed by MacCorquodale and Meehl (1948) and regard the HC as *de facto* operative in current theory. The theoretical endeavor is envisioned as a gradual purification of HCs from the theoretical language so that the ultimate term of the theory is the formulation of purely functional IVs unambiguously linked with each other and finally to the empirical variables. In this framework, the HC is regarded as a formulation transitional to the terminal IV (George, 1953). The differentiation is made in terms of operational validity. The HC is characterized as a somewhat vague and non-operational concept. It often includes elements borrowed from common sense or common experience, and its significance is blurred by this imprecision. The IV, however, possesses a high degree of operational validity. The precise determination of elements in operational terms permits the rigorous deduction and testability which is required for meaningful scientific usage. The process of theoretical refinement consists in the progressive determination of the blurred terms of the HC by operational definition (Marx, 1951a). The criterion of operational validity is the number of operations involved in the definition of each term (George, 1953). As the common sense terms are replaced by operational definitions, the concept moves from the status of HC to that of IV. Consequently, the HC is a temporary, transitional, and sometimes helpful moment in progressive elaboration and formulation of scientifically rigorous and operationally defined IVs (Marx, 1951b).

The question we are asking at the moment is whether this schema, centering on the primacy of the IV, enjoys an exclusive validity in psychological explanation. The point is not whether psychological theories have, in fact, achieved the highly operational validity implied in the purely IV schema, but rather whether the IV schema ought to be set up as a theoretical objective. The most extensive attempt to formulate a theory in these terms was that of Clark Hull, but the careful analysis of his theory by Koch (1954) has made it quite clear that Hull was unable to carry out the methodological program to which he directed his efforts. Koch has shown that Hull's Type I IVs (*s*, *r*, *D*), which are directly linked to independent variables, have strong physiological implications. For example, *s* is defined as the "afferent neural impulse resulting from the action of a stimulus energy on a receptor" (Hull, 1943, p. 407). Type II IVs (*sHr*, *sEr*) are indirectly connected with physiological elements through the intermediate IVs of Type I, and Type III IVs (*sOr*, *sLr*) are quite independent of the previous types. Consequently, all of Hull's IVs are involved in physiological reference except the Type III variables; and the Type III variables are completely outside the scope of Hull's methodological principles and metatheoretical commitments since they are not linked to any antecedent observable conditions. When we remember that Hull's method involved a tailoring of the problems to the method and a tailoring of the method to the problem, we have good reason to question the practical feasibility of the methodological ideal.

Even in Hull's highly sophisticated analysis, the IVP failed to attain high operational validity. Hull's mediating variables are effectively HCs (Meissner, 1960). It is not clear that Hull conceived them as such. The mathematical formulations of many of his constructs are decidedly and intentionally functional. Subsequent redefinition in verbal terms carried with it the reference to entities which adulterated the purity of Hull's original concept. In the subsequent attempt to apply these concepts in the analysis of behavioral situations, the denotative character was fixed. Equivalently, Hull's IVs were transformed into HCs—and the transformation was never subjected to methodological scrutiny.

That a theorist of Hull's calibre should have failed does not argue to the invalidity of the IVP in principle. It seems reasonable to presume that any argument, which would attempt to show that psychological theory can not be reduced to IVs and their connection to observables, would have to be based on an analysis of the relation of IVs to empirical content or on the nature of scientific theory itself. In the first instance, if it can be shown that the meaning of theoretical terms can not be adequately formulated in empirical terms, we would have grounds for arguing that the IV might not be as effective as a term of scientific analysis as had been thought. This aspect of the problem has been taken up by Hempel in what he calls the "theoretician's dilemma" (Hempel, 1958). The dilemma is this: since the terms and principles of a theory establish definite connections among observables, they can be dispensed with since they are replaceable by a law which links antecedent observables directly with consequent observables. Hempel rejects the dilemma because the conclusion, which asserts the substitutability of lawful connections of observables for theoretical terms, rests on a false premise. In terms of our own problematic, we can conclude from this that there are no logical grounds for maintaining that theoretical terms (HCs) can be replaced by statements of functional connections of observables (IVs) without loss of meaning. On the other hand, the logical argument does not permit the conclusion that theoretical issues can be handled adequately in terms of HCs. Nor does it permit us to conclude that the IV is without validity. It does, however, cast doubt on the methodological primacy which has been attributed to the IV.

However convincing and significant the logical argument may be, I believe a more basic argument can be found in terms of the nature of theoretical activity. To argue about the adequacy or inadequacy of definitional status of theoretical terms is actually a secondary issue. Even if the argument were conclusive, the methodological issue would remain untouched. If complete reducibility of theoretical terms to empirical bases is established, the theorist is still left with the problem of determining whether the goal of theory ought properly to be to strive for definitional completeness or whether the construction of theoretical terms on incomplete definitional bases is to be preferred. On the other hand, if complete irreducibility were to be established, the theorist

is still left with the problem of determining the most effective means to attain his theoretical goals. Consequently, it would seem that the theoretician's dilemma is a secondary one and that Hempel's argument, however important and convincing, is not conclusive.

In the second instance, the argument for the irreducibility of psychological theory to IVs might be based on an analysis of the nature of scientific theory. The nature of theory in psychology is too profound a question to be treated in any summary fashion. So we shall confine this consideration to merely sketching the points of connection between that far-reaching issue and our immediate concern of the primacy of the IV.

One of the basic assumptions of the IVP is that there is a methodological continuity between the HC and the IV. We have already noted the transformation of IVs into HCs in Hull's theory. One might be tempted on the grounds of such evidence to think that the transformation was continuous; but the direction of Hull's transformation is opposed to that which we are considering at the moment. The IVP seems to regard the HC as a preliminary stage in the evolution of operationally defined IVs. The supposition is that the vague and essentially non-operational HCs are gradually translated into IVs by successive precision in terms of operational linkages. This supposition has already been called into question (Meissner, 1960). It does not seem adequate to the distinction to conceive the HC-IV dichotomy in terms of a continuum based on the specificity of operational definition. In terms of theoretical origin, the IV is constituted by the mere expression of empirical established regularities in functional form. The method is essentially abstractive: empirically measured relations are set in a quantitative expression which is determined by the mathematical characteristics of the relationship in question.

Hull's (1943) construction of the relation of sHr to N (number of reinforcements) is instructive. Habit strength is not open to direct observation and consequently must be inferred from observables. There are two groups of observables connected to sHr: antecedent conditions and consequent behavior. Hull then selects four empirical measures of consequent behavior which he regards as pertinent to the sHr-N connection: amplitude as a function of number of reinforcements, latency as a function of repeated trials, resistance to extinction as a function of the number of reinforcements, and the probability of correct responses as a function of "successive hundreds of responses." Using the experiments which yielded these measures as a point of departure, Hull concludes that sHr is an increasing function of N and that the function increases up to a physiological limit. As sHr approaches the limit, the increment of sHr from each additional N decreases progressively. Hull summarizes these relationships and the assumption of a growth function in the equation:  $sHr = M - Me^{-iN}$ , in which  $M = 100$ ,  $e = 10$  and  $i$  is a logarithmic function of the reduction constant. The method is peculiarly illustrative of Hull's method of obtaining quantitative func-

tions. He recognizes the difficulties in the indirect inference of linking IVs with their systematic independent variables. He chooses four functional relations between the directly measureable antecedent N and the consequents of sHr. Three of these relations are fitted with curves which turn out to be growth functions and Hull concludes that the theoretical relationship between sHr and N will have to take the form of a growth function. As Koch (1954) points out, Hull has inferred an IV function between the IV and a systematic independent variable from an experimental function obtained between independent and dependent variables. Hull was pushing too far and too fast. If his procedure had been fully legitimate he would have left the growth function relation between the antecedent N and the consequent measures as the summary expression of sHr. However, Hull's goals may have been more programmatic than systematic, at least in the present instance.

The essential point to be made about the IV is that methodologically nothing is introduced into the structure of the relationships which has not been explicitly linked to observables and which can not ultimately be reduced to the empirical variables. Consequently, specification of meaning through operational definitions will consist in the precise statement of the empirical elements which the IV summarizes and expresses. In this sense, the IV is fully reducible to observable terms. The case of the IIC is clearly different. Given empirically established relationships, the theorist formulates the HC in an attempt to find the reason for the relationships involved. The formulation is not merely abstractive; it is constructive. The construct adds meaning to the theoretical structure which cannot be reduced to empirical variables and which is not properly derived from the variables. The HC, therefore, contains a surplus meaning which is derived, not from the empirically established relations, but from extrinsic evidences or from the creative scientific imagination of the theorist. The function of the HC is clearly distinct from the function of the IV. Rather than the functional statement of relationships, the HC aims at the development of an explanatory structure which will account for the empirically established relationships. The question, then, comes down to this: Can the HC be resolved into an IV by the gradual operational reduction of its meaning to empirical referents? The basic reason for our negative reply is that the HC is essentially an explanatory construct. Any good explanation conveys more meaning than that which it explains—otherwise it is not an explanation at all. If the HC does not fulfill this explanatory function, it is equivalently an IV. To reduce the HC to an IV would seem to be equivalent to reducing an explanation to that which it explains. To do so would be to eliminate the explanation.

Again, an example may clarify this point. Hebb's (1959) neuropsychological theory of behavior is built around the key concepts of *cell assembly* and *phase sequence*. Independent variables may include various patterns of sensory excitation, direct neural stimulation, internal conditions of the blood stream and plasma, and various structural prop-

erties of the nervous system under varying conditions of heredity, disease pathology or surgical injury. Dependent variables are muscular and glandular activity, EEG, GSR, or even the movement tendencies of the whole organism. The grounds of the theory are not quantified to any appreciable degree and they are basically physiological. The experimental relationships which have been established among these variables, both because of the qualitative condition of the information and because of the theoretical objectives dictated by Hebb's interests and the requirements of the scientific community, are not subjected to functional organization. Rather Hebb's aim is to find the reason for the patterns of relation which emerge from his experimental work. Because it is dealing with physiological data, the explanatory construct or HC is given a physiological structure. The cell assembly and its higher integration into phase sequences are not proposed as summary statements of the experimental relations. They are proposed as explanations of these relations and as such they tell us something which is not properly discoverable in the relations themselves. It is on this basis, incidentally, that the HC is so often appealed to as more fruitful than the IV in terms of stimulating research.

If we can safely say, then, that the IV and the HC constitute two distinct moments in the scientific enterprise and that the methodological context of each is independent and quite different, we can turn our attention to the focal problem of the relative functional validity proper to each. I do not think that an answer can be given in any simple and categorical fashion. Most theorists have a broad enough frame of reference to recognize that both types of concepts have a place in psychological theory. The question is: What place does each properly hold? Most psychological theorists who start with the IVP as a theoretical framework tend to subordinate the HC to the IV. It is easy to see that in a case like Hull's attempt to establish a functional relation between sHr and N one might regard the shift of sHr from a purely functional IV to an HC as a failure to carry through methodological commitments with sufficient rigor. In other words, one might feel that once Hull had deviated from the line of strict logical development in formulating his concept of sHr as an IV, he might readily have compensated this deficiency by converting sHr into a convenient explanatory construct. Once this shift had been made, it was relatively easy to use the sHr in a quite fluid way in terms of neural structure or even attaching to it a certain experiential reference (Meissner, 1958). The added consideration that the HC is the predominant type in more qualitative theories and that quantitative precision, which is regarded as a mark of sophistication in theory, is more often found in theories which profess to be forming IVs, has led to the assumption that the HC is somehow an inferior type of theoretical term.

Theoretical construction is a somewhat amorphous enterprise. Its objectives can not be specified in any simple terms. The theorist may be seeking understanding, he may be trying to explain, he may be trying to merely summarize and organize large mounts of data in a convenient

way, he may be organizing observed regularities or lawful sequences into higher levels of organization, or he may be trying to provoke research through fruitful hypotheses. The objectives of theory construction can not be adequately specified by any one of these, and I would hesitate to say that it was adequately described by them all. In any case, the course of development which theory construction will take depends in large measure on the goals which the individual theorist has set for himself.

We have suggested that the ultimate direction of theoretical construction must be governed by the immediate and long-range goals of a given theorist. At certain points in the development of a theory, organization of data and the clarification of functional relationships may be the preferable objective. Where the establishment of such precise relationships is either unfeasible or can only be had in relatively insignificant functions of unimportant scope, the preferable theoretical course may lie in the direction of hypothetical construction on the basis of more or less qualitatively established relationships. In this sort of immediate theoretical context, which is often the case in psychology and in the social sciences in general, hypothetical explanation may serve a more stimulating function for experimental discovery and confirmation of data which will, in turn, permit greater precision in the quantitative statement of the relationship among the variables.

Let us suppose, however, that highly precise quantitative relationships have been established and experimentally confirmed. Let us further suppose that a theoretical structure has been developed in terms of IVs. The theory, in this case, would provide a powerful predictive tool and would be very useful indeed. But one would be a very poor specimen of a scientist if he were content to let it go at that. The scientific intelligence cannot satisfy itself with abstract functions which remain extrinsic to the object of investigation, even though they provide a powerful means of control of that object. If I may draw for a moment on other areas of scientific concern, the same type of dynamism can be found in physics. Starting from the experimental work of Boyle, Charles and Gay-Lussac and the laws governing temperature and pressure which they established, Joule, Mayer and others developed a theory of thermodynamics. The theory is concerned with functional relationships between properties of materials which can be quantitatively determined: specific heats, coefficients of expansion, etc. Thermodynamics is essentially a theory of the IV type. The scientific intelligence could not stop there, however; it had to explain the established relations. The explanatory constructs which have been developed to account for thermodynamic equations followed two distinct lines: the kinetic theory applies the laws of mechanics to individual molecules of a system and from these it derives expressions for gas pressure, specific heat capacity, etc.; the second line of approach was through statistical mechanics, which applies probability methods to large numbers of molecules. Both approaches are in terms of explanatory constructs (HCs) based on the model of molecular particles. A



similar case can be found in chemical explanations of quantitatively precise laws of chemical combination by means of atomic models, valences, electronic structures, etc.

If we conceive the HC in this manner, the conclusion begins to emerge that the HC has a definitive role to play in psychological theory. Our knowledge of behavior has not reached the level wherein this definitive role becomes operative to any great extent. However, it is clear, even in psychology, that the HC is methodologically distinct and independent of its opposite number, the IV. The functional validity, therefore, of both HC and IV is relative to the theoretical concerns of the investigator, the immediate or long-range goals of his theoretical activity and the availability of certain types of data. As long as it is scientifically useful to organize theory within the framework of the IVP—and as far as psychology is concerned, this assumption is not beyond question—the theorist ought to be left free to direct his creative scientific thinking to the scientific goals which he has determined. Theoretical activity, as a strictly scientific procedure, is subject to certain limits, which are determined on the one hand by the structural properties of the object of investigation and on the other by the needs and demands of the scientific community. Constriction of conceptual patterns to one or other type of intervening construct is an unscientific limitation of the essential scope of scientific thought. Psychology, more than any other science, must look to the creative flexibility of her theorists for the better understanding of human behavior.

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## STRESS AND ANXIETY AS HOMOMORPHISMS

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Current usage of the term "anxiety" in the psychological literature varies with the measuring instrument employed. For some, these measures are merely correlates of anxiety, a position that assumes anxiety to be an ideational state which causes the physical symptoms; for others, the response measures themselves are "the anxiety," the operationists's position. Three classes of anxiety correlates or measures are now in vogue—autonomic nervous system (ANS) and other physico-chemical changes, somatic nervous system (SNS) changes and subject-report. Among the ANS and other physico-chemical responses studied are salivary output and pH, cardiac and circulatory changes, skin resistance and palmar sweating, blood chemistry and other blood changes, and brain potentials. Some SNS variables which have been investigated are tremors and muscle tension, and the method of subject-report has employed both oral and written responses on ideational and physical states. The last method has been used more and more frequently following the introduction of the paper and pencil anxiety questionnaire (Sarason & Mandler, 1952; Taylor, 1953), which was accompanied by the belief on the part of many that psychologists now had a fast, efficient way to measure anxiety with a minimum of contaminating stimuli such as apparatus harness. Controversies have subsequently been waged over the generality vs. specificity of anxiety, the proponents of the latter position demonstrating the less than 1.00 correlation between the Manifest Anxiety Scale (Taylor, 1953) and the Test Anxiety Questionnaire (Sarason & Mandler, 1952). Other investigations have been concerned with the effect of anxiety, as "measured" by paper and pencil questionnaires, on performance. A review of these and other studies illustrates that the concept of anxiety is being used in a very loose and variable way. The anxiety questionnaires are hodgepodes of statements pertaining to stimulus (situational) and response (reaction) variables, a simple additive model being assumed for statements which may be quite different in their relations to other (criterion) variables and which have numerous semantic pitfalls. Considering the above, it would seem appropriate to examine the concept of anxiety and to attempt to define it and related variables more rigorously, or at least to sketch a program concerning how this may be done, with the hope that research on anxiety will be designed more adequately and interpreted more meaningfully. However, before disputing over the meaning of anxiety, it may be fruitful to introduce some of Selye's (1956) thinking on, at least for the present writer, the more comprehensive concept of stress.

Although a cursory examination of Selye's definition of stress leaves one with the feeling that the former is both inconsistent and circular within the same book, a patient analysis of the following statements reveals what Selye is talking about (Selye, 1956):

(a) I therefore propose to substitute the word stressor for the agent and retain stress for the condition (p. 41).

(b) The word stress designates the sum of all the non-specific effects of factors (normal activity, disease-producers, drugs, etc.), which act upon the body. The agents themselves are called stressors, when we refer to their ability to produce stress (p. 42).

(c) Stress is an abstraction; it has no independent existence (p. 43).

(d) In a nutshell, the response to stress has a tripartite mechanism, consisting of: (1) the direct effect of the stressor upon the body; (2) internal responses which stimulate tissue-defense; and (3) internal responses which stimulate tissue-surrender by inhibiting defense (p. 47).

(e) Stress is the state manifested by a specific syndrome which consists of all the nonspecifically induced changes within a biologic system (p. 54).

(f) The stressor is "that which produces stress" (p. 64).

Thus stress is a state or condition but not a measurable variable. In Selye's system, stress is conceptualized as an intervening variable, while "stressor" is defined in terms of "response to stress." Thus, in Selye's research the discovery of the "response to stress" was propaedeutic to the identification of stressors. From previous research, of course, we have some notions about stressors, and in an experimental situation some stimulus must be applied to the organism if a response is to occur. But the emphasis of the present paper is that we must first discover stable and characteristic response patterns and then find out what leads to them.

In a recent paper, Hunt, Cole, & Reis (1958) list five criteria for identifying emotions: "(1) the overt responses or expressions, (2) the inner organic, neural, and visceral changes or indicators, (3) the arousing situations, (4) the emotional experiences, and (5) the motivational effects" (p. 146). Hunt, Cole, & Reis (1958) and others (McClelland, *et. al.*, 1953) base their disenchantment with criterion (1) on studies which failed to demonstrate relationships between judgments of emotion from overt expression without knowledge of the stimulus (e.g. Sherman, 1927) and from studies which failed to reveal differential overt response patterns in subjects when exposed to stimuli which presumably should give rise to different emotions (e.g. Landis, 1924). Although the methodology of the last two studies has been criticized elsewhere (Woodworth and Schlosberg, 1954), the writer will return to a discussion of the experimental evidence for the situational approach advocated by Hunt, Cole, and Reis later in the paper. The suggestions which follow for defining stress and anxiety represent an adaptation of Selye's (1956) writings.

The term "stress reaction" will be used in a manner similar to

that of Selye in his definition of response to stress (Selye, 1956, p. 47) (see also definition (d) above). However, Selye's definition includes only physico-chemical responses, but the definition program proposed here includes motor and ideational patterns as well. A formal definition of this tripartite pattern is purposely omitted since patterns of stress reactions are what must be initially discovered. Obviously, certain components of patterns (groups of highly inter-correlated measures) which might be labeled stress reaction patterns have already been discovered, e.g. respiration rate and muscular tension (Telford & Storlie, 1946), but many more components of such patterns or many more patterns undoubtedly exist, and formal definition at this stage would needlessly exclude these. However, one need not await the discovery of the entire pattern or group of patterns in order to identify antecedents. Although it is proposed that the discovery of R-R relationships is logically prior to the identification of S-R correlations, when a cluster of responses exists it is not necessary to await the inclusion of other responses into the cluster in order to investigate its antecedents; it would be wise, however, to assure oneself of the stability of the interrelationships among the pattern components and the reliabilities of these components before proceeding further. Once this has been done, the search for stressors begins, the term stressor being applied to any stimulus or stimulus pattern which is reliably and contiguously precedent to a stress reaction.

A simple illustration should clarify the above. For example, if increase in heart rate is labeled as a stress reaction, then any of the following may be stressors: the perception of a wild animal on the loose, the injection of adrenalin into the blood or exciting news from home. Thus, it is proposed that there is no one-to-one relationship (isomorphism) between stressors and stress reactions. The mapping of stressors onto stress reactions for a given organism at a given time is undoubtedly many-to-one (homomorphism), to use an analogy from mathematics; the mapping paradigm is called stress. It is clear that this method of definition makes stress a very broad, second-order concept. In fact, all of the "emotional" responses (anger, fear, sorrow, joy, etc.) and emergency reactions, or any reaction which indicates that the organism is in a state of metabolic or psychological disequilibrium and begins to dip into stores of energy are classified here as stress reactions (see Cannon, 1929). The common characteristics of many of these reactions are evidenced by studies in psychosomatic medicine which have demonstrated that the organism responds with the same physiological reactions to numerous stimuli (Wolff, 1953); likewise, motor reactions to different stimuli are often indistinguishable to observers (Sherman, 1927). By studying the relationships among the various components of the three classes of variables which we have labeled stress reactions, a number of distinctive, but perhaps overlapping, stress reaction patterns will emerge.

If a satisfactory solution to the problem of measuring the response

variables is found, then one way of discovering both generalized and group patterns would be to take, say, 15 or 20 different response measures, following the onset of a stimulus known from prior experience to produce an intense reaction, on a large number of individuals and then factor both the matrix of intercorrelations of persons over tests (Q technique) and the matrix of intercorrelations of tests over persons (R technique). The components of the emerging patterns could then be weighted by the factor loadings and the equations related to other antecedent variables (stressors). Apropos of the above, studies which have been concerned with individual differences in reaction to the same stimulus (Funkenstein, et al., 1952; Lacey, 1956; Rosenzweig, 1934; Wenger and Gilchrist, 1948) suggest that the Q-technique of factor analysis may be very useful in this area.

To date, investigations (Funkenstein et al., 1952; Rosenzweig, 1934) have found at least two reaction patterns—the intropunitive (anger-in) and extrapunitive (anger-out). Such patterns consist of highly inter-related ideational, motor, and physico-chemical reactions. It may be that one of these three classes of reactions has greater reliability than the others. For example, a person may not run away when confronted with a certain stimulus pattern although his autonomic nervous system is in a state of "emergency." In addition, this person may even state to others that he is not afraid, but he says to himself that he is. It follows that although some of the reaction components may be absent when others are present, if the present components have been highly associated with the absent ones in the past there is justification for labeling the part-reaction as a stress reaction. Greater weight is given to more reliable evidence.

Once a stress reaction pattern has been defined and the stressors identified, then the question of the functions of the reaction pattern may be posed. The reply to the theoretical query may be found in the answer to an experimental question: What will be the outcomes if we expose the organism to the stimulus named a stressor and control the appearance of the stress reaction pattern? If there is a measurable difference in outcome in the two cases of non-appearance and appearance of the reaction pattern, the "function" of the stress reaction patterns is to avoid those outcomes which result in the first case but not in the second. If intensity reduction or elimination of a stimulus pattern which was introduced onto or into the organism occurs in the second case but not in the first, then one function of the stress reaction is to modify the given stimulus pattern. However, this may be only a superficial function. Certain stimulus patterns may be learned or native "signs" to the organism of other stimulus patterns or states. If this is known, then the "basic" function of the stress reaction is to avoid the latter or ultimate state.

It would be difficult if not impossible to control the appearance of the entire stress reaction pattern, especially the physico-chemical

components, without, of course, controlling the stressor. However, it is quite feasible to control the appearance of motor, and perhaps ideational, components of the pattern. To take a simple case with an obvious answer. What happens to me if I stand in front of a charging bull? What happens if I stand aside or run away? In this case, one might hazard that the difference would be one of intactness of my body. A more laboratory oriented example would be to restrain or immobilize an animal with curare and observe what happens to the animal when exposed to stressors and non-stressors. Thus, by manipulating components and observing outcomes, one may make assumptions about the function of the pattern as a whole.

It is obvious that a stress reaction pattern may be varied according to intensity and duration, as well as appearance, perhaps by variation of the stressor along the intensity and time dimensions. Consequently, it may be objected that since a stress reaction has different outcomes when components vary in degree and duration, one function of the reaction may be to injure the organism (stage of exhaustion) (Selye, 1956). Therefore, no matter how many studies witness the preservative or protective functions of the reaction, there is a glaring contradiction in a theory which states that the basic function of a stress reaction is the preservation of the organism. But from a logical standpoint, there is no difficulty in conceiving of a reaction pattern as having different functions in different states, as long as function is not equated with purpose. For example, one may choose to study the functions of a stress reaction within a fairly narrow range of intensities and durations and make generalizations within specified limits. This is common procedure in psychology (Weber-Fechner Law) and in other sciences (Hooke's Law).

In order to study the effect of a stress reaction on other behavior, viz., performance, the design is much easier to effect. Once certain stimulus patterns have been identified as stressors for a given organism, then the appearance and magnitude of these stressors may be controlled and the difference in performance measures observed. If so-called universal or generalized (for a given species of organism) stressors have been identified, then one has justification for doing the experiment with a group of such organisms and drawing conclusions about the effect of the stress reaction pattern on the performance of the whole species.

Other experiments may be designed to study the modification of a stress reaction or the effectiveness of a stressor through learning. In this connection, it should be clear that when a stress reaction no longer occurs following the introduction of a given stimulus, that stimulus is no longer a stressor for that organism. Stimuli may also become stressors through association with prior stressors, and different reaction patterns, either stress or non-stress, may result from such stimulus pairings. It would seem, then, that the initial function of research in

the area of stress should be to discover reliable stress reaction patterns and then to identify the stimulus complexes which are followed by such reactions. Then one may do experiments to ascertain the effect of stress reactions on other behavior by controlling the appearance of the stimulus complexes.

In summary, we begin with the outline of a program for defining stress reactions in terms of physico-chemical, motor, and ideational responses. Those stimuli or stimulus patterns which are reliably and contiguously precedent to such stress reactions in a given organism are termed stressors for that organism. The second-order concept of stress is applied to such stressor-stress reaction sequences. With these ideas in mind, we are now prepared to consider the concept of anxiety.

The meaning of the term anxiety has been particularly difficult to tie down, and the diversity of connotations which it holds in common language has not helped the situation. However, rather than reject the term altogether, it may be more useful to attempt a redefinition. The following introduces anxiety as a variety of stress. Parallel to the treatment of the concept of stress, the writer will use the term "anxor" for stimulus and "anxiety reaction" for response, making the term "anxiety" a higher order concept to refer to anxor-anxiety reaction sequences. An anxiety reaction may be tentatively defined as a response pattern consisting of the following components:

*Motor reaction*—attempt to withdraw, tremors, muscular tension, and associated SNS reactions.

*Ideational reaction*—"I feel nervous, upset, anxious, worried, apprehensive" or equivalent (synonymic) statements.

*Physico-chemical reaction*—excessive perspiration, accelerated heart beat, increased respiration rate, galvanic skin response, increase in gastric secretion, and other related physico-chemical reactions. The higher the intercorrelations among these three classes of responses, the more secure the concept of anxiety reaction. The anxiety reaction may follow the onset of any number of symbolic or physico-chemical stimuli. These are the anxors. Note that no distinction between anxiety and fear is made.

A word on the relationships between stress reactions and performance seems in order. If other behaviors of the motor or ideational class are found to invariably accompany a stress reaction pattern, then it is legitimate to consider these behaviors as a part of the reaction pattern. Thus, if loss of spontaneity, deterioration of thought and judgment, and poorly controlled actions occur when an anxiety reaction pattern occurs, then the former may be included in a redefinition of the pattern. It should be noted that the identification of new components of a stress reaction pattern will probably merely expand and nowise impugn re-



search employing the earlier, delimited pattern, since there is a high degree of transitivity among highly intercorrelated variables.

Returning now to the discussion of the experiments of Hunt, Cole, and Reis (1958), in the words of the investigators:

In the first experiment, the differential cues were formulated in terms of the "perceived timing" of the frustration: present for *anger*, future for *fear*, and past for *sorrow*. The hypothesis was tested by presenting 30 descriptions of frustrating situations, 10 for each perceived timing, and asking S to choose either *anger*, *fear* or *sorrow* as the names for the emotions that they would feel in the situations as described. Considerable agreement among Ss was found, but correspondence between the emotion named and perceived timing was not high (p. 150).

In a second experiment by the same investigators, the formulation concerning the nature of the differentiating cues was changed, but this "had no significant effect on agreement for situations intended to evoke *fear* and *anger*, but did increase somewhat the percentage of *sorrow*-responses to situations intended to evoke sorrow" (p. 150-151). Now although the results of these experiments did not offer great support to the hypotheses, the investigation is an example of studies which hypothesize perceived stimulus differences as distinguishers of emotions. However, even if the distinction among the emotions should turn out to be primarily cortical in nature, since the body reaction is so similar for many different emotion names, and even if such cortical distinction should turn out to depend on perceived timing of frustration (which it probably will not) this does not invalidate the potential fruitfulness of the program sketched in the present paper. Perceived timing of a stimulus (stressor) is an ideational response. Certainly the perceived timing, magnitude, duration, etc. of the stimulus all effect its efficacy, but these variables are more complex and numerous than physico-chemical and motor components of reaction patterns. Thus, it would seem that more "spade" work employing the last two types of variables should be prior to an analysis of meaning.

The author should state his realization that the approach suggested above is not new. It is primarily an attempt to state clearly what many others may have taken for granted and should be construed as a reaffirmation of faith. Woodworth and Schlosberg (1954) present an excellent review of studies which have attempted to isolate both generalized and group patterns of emotional response. The extensive research by Lacey (1956) on autonomic measurement and autonomic patterning should also be cited. Studies which have failed to reveal relationships among various measures are justifiably criticized as employing inadequate methodology or measurement and hence do not rigorously test for stress reaction patterns. However, promising results have been obtained in several studies employing correlational and factor-analytic methods (e.g. Wenger and Gilchrist, 1948). Finally, the writer does not intend to belittle the problems involved in deciding what to measure

and how to measure it. Both methodological and measurement problems will require much serious thought prior to effecting any program such as that outlined above.

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## PERSPECTIVES IN PSYCHOLOGY

XIX. PRIVATE EXPERIENCE REVISITED<sup>1,2</sup>

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A very important concept in science, especially psychology, is the concept of private experience. One of the major problems revolving around this concept is its definition. It will be the purpose of this article to examine it critically and to suggest a definition that may resolve some of the difficulties that have developed around this concept.

The concept of private experience continues to be one of the most elusive constructs in modern psychology. Reference to it as a critical construct is made by psychologists with widely divergent interests. Frank (1939), in discussing the Rorschach method, indicates the importance of private experience by stating that "the Rorschach method offers a procedure through which the individual is induced to reveal his 'private world' by telling what he 'sees' in the several cards . . ." It is interesting to note that Frank places quotation marks around the word "sees." The word "sees" suggests that the individual is having an experience that is private to him. The "private world" that Frank mentions probably alludes to the unconscious rather than the conscious experience implied in "sees." Boring (1945), on the other hand, has indicated that the construct, private experience, is one of the constructs in psychology which is in greatest need of being operationally defined. Thus, we find both the clinical psychologist and the experimental-theoretical psychologist emphasizing the importance of the same construct. It is not too often that representatives of these two groups agree.

## PRIVATE EXPERIENCE IN EMPIRICAL SCIENCE

Marx (1951), in discussing the sociolinguistic aspects of science, points out that "from this point of view (the necessity of producing a protocol of observations by either the experimenter or the clinician) the special status of 'private experience' or 'consciousness' as a uniquely psychological subject matter—the problem of 'existentialism'—is not regarded as a scientific question, since an answer one way or the other does not make any difference in the kind of scientific work that needs

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to be done." Reichenbach (1951) tends to agree with Marx when he contends that to the scientist studying kangaroos the question of the reality of the kangaroos is immaterial. However, some psychologists do not entirely agree with Marx on this matter. McClelland (1955) reported a new approach to the analysis of mental content. He pointed out the sterility of other methods of investigating mental content and proposed a new mode of attack on an old problem. Bakan (1954) also pointed out the inadequacy of the historical introspective technique and suggested a new approach to the technique which, according to Bakan, portends the opening of new and challenging fields of psychology. Moreover, there are indications that private experience is not a concept unique to psychology. Bridgman (1928) contends that "in the last analysis science is only my private experience." The contention that science reduces to private experience and that private experience is the domain of psychology led Stevens (1936) to contend that "psychology is the propaedeutic science." Bridgman's contention about science and Boring's plea for an operational definition of private experience suggest that the concept of private experience is a crucial one, not only for psychology, but for all sciences. It is the purpose of this paper to analyze the concept of private experience and suggest a resolution of the difficulties associated with this concept.

#### OPERATIONISM AND PRIVATE EXPERIENCE

Wundt (1901) made the distinction between physics as mediated experience and psychology as immediate experience. In one form or another, this distinction has prevailed to the present time. In spite of the high hopes held by the proponents of operationism to rid psychology of its metaphysical constructions, there has been strong resistance to modification of constructs which have had status for many years. It is not impossible, as Skinner (1938) has pointed out, to operationally define any construct. In spite of Skinner's observation about the susceptibility of constructs to be operationally defined, there is objection to defining constructs in this way. Prentice (1946), for example, claims that Krech destroyed the major value of the construct, hypothesis, when he operationally defined it. Before bowing to the demands of operationism, Krech had been able to note a facet of behavior and account for it through the construct of hypothesis, according to Prentice. However, when Krech operationally defined hypothesis, he "defeated his own cause by his over-zealous operationism . . ." The apparent basis for Prentice's objection stems from a philosophic realism which suggests continuous modification of definitions until they approach more closely the reality represented by the construct. It is noteworthy that no clues are provided so that reality will be recognized when it is attained.

Boring (1945), in his plea for an operational definition of private experience, seems to have ignored his operational definition of intelligence on the one hand and Bridgman's conception of an operationally

defined construct on the other. It appears that an essential characteristic of operationism is its emphasis upon a consistent arbitrariness. A given set of operations can be given any particular name, but once the name is given, the set of operations defining the name is established. If the set of operations is changed, then it would be advisable to change the name by which the set of operations is designated. This procedure would avoid considerable confusion. Accordingly, an operational definition of private experience will be that set of operations to which is applied or designated the term, private experience. This conception of private experience will be objectionable, just as there are objections to Boring's operational definition of intelligence, because it may not include those facets of behavior which are implicitly subsumed under private experience. However, one of the major values of operationism is destroyed if definitions are not limited to the set of operations that have evolved for a particular construct. One may be able to say that it is possible to develop a definition of intelligence that will be more useful than Boring's, but one cannot contest the legitimacy of his definition of intelligence on operational grounds.

A major difficulty appears to derive from the excess or surplus meaning of an operationally defined private experience. Instead of pursuing an operationally defined construct to its limits, psychologists have tended to reject the operational definition primarily on the grounds that excess meaning has been eliminated. That is, operational definitions are rejected because they tend to eliminate, or at least reduce, excess meaning. It appears that operationism has assumed two diametrically opposed functions. For some psychologists it is a monster that is being crammed down the throats of psychologists, such that the very heart is being ripped out of psychology. For other psychologists, operationism is a panacea that will cure all the ills of psychology. It is highly doubtful that any methodological procedure could perform either function. Operationism is a methodological procedure for the defining of constructs. It is not the function of operationism to designate those facets of a situation which must be included within the definitional framework to make the concept an operationally defined one.

#### A FURTHER ANALYSIS OF PRIVATE EXPERIENCE

A further analysis of the concept of private experience indicates that there must be privately occurring events which determine the behavior which is subsequently observed. Private experience then seems to be a series of events occurring between S and R which modify S and determine R. This position appears to be the one held by Stevens (1939) in discussing observations as discriminations. The question arises concerning the kind of processes which are involved in this sequence of operations. Hull (1943), does not clearly specify the processes when he says, "The subjective aspect of experience is dependent upon the private nature of the process hidden within the body of the subject;

subjectivism in the field of behavior theory, on the other hand, is dependent upon the private nature of the processes within the body of the theorist, whereby he attempts to explain the behavior of the subject." What is the nature of these processes hidden within the body? Are these processes neurophysiological, biochemical, bioelectrical or some other measurable processes? There is little evidence that these processes are related to neurophysiological, biochemical or bioelectrical operations. It appears that most of these private processes are psychical in nature.

Skinner (1953) presents an interesting approach to private experience. Private experience is defined primarily in terms of availability of stimuli associated with the observable response. If the stimuli are available to the community, there is public experience. If the stimuli are available only to the individual, then there is private experience. Thus, the stimuli for the toothache exemplify private experience. Though there may be merit in this distinction that Skinner makes between public and private experience, it is possible that the verbal or nonverbal behavior which occurs may be under the control of stimuli that are accessible to the public. The response, "I have a toothache," is under the control of stimuli that are available to a dentist. If these stimuli were not available to the dentist, it is improbable that he would be able to take actions that will reduce this response probability to zero. When the stimuli for behavior are not accessible to the community, there is a high probability that the individual will be diagnosed as abnormal. Jessor (1956) has made the interesting point that all conceptualizations of stimulus in psychology, and other scientific areas, also involve the concept of response. The S-R psychologists tend to utilize physicalistic responses as their basis for stimulus. It would appear that Jessor contends that S-R psychology is actually an  $R \rightarrow S \rightarrow R$  psychology. The first R is the response of the organism, usually the experimenter, to some instrument that provides information through physicalistic measurements. On the basis of this R, E then creates S to which another organism may respond. On the other hand, his proposed phenomenological approach appears to be an  $R \rightarrow R \rightarrow R$  psychology. The second R reflects the interaction of the organism and his private experiential life. It appears to the writer that a psychical flavor is introduced into Jessor's conceptualizations. However, the admission of a psychical foundation of psychology would produce a dualistic psychology which Bills (1938) claims has been eliminated from psychology. At the same time, psychologists have neglected to state the drawbacks to a mentalistic or dualistic psychology. Kantor (1958) has contended for years that mentalism has not been removed from psychology, but the inclusion of mentalism has become much more subtle. Ryle (1950) has presented a very formidable array of bases for rejecting a dualistic position in psychology. A dualistic position in psychology makes the postulate of uniformity in nature untenable. Psychology, by making a dualistic assumption, would be out of step with the natural sciences which do not make

such an assumption. Secondly, a dualistic assumption introduces or encourages a belief in a rational basis to psychology, a position which would severely damage the development of a science of behavior.

### A RESOLUTION OF THE PROBLEM

We seem to have reached an impasse in the psychological Gordian Knot of how to make private experience public. Boring has indicated that all of science is based on private experience, so one would presume that the future development of psychology requires the unraveling of this perplexing riddle. It is entirely possible, however, that this noose around the neck of psychology has been put there by the psychologists themselves. In other words, psychologists have made the assumption that there is "really" a private experience which is basic to all observations. This assumption need not be made. Psychologists are not bound by any of the data of psychology to assume an underlying or basic pool of private experience, although Bergmann and Spence (1941) readily concede that private experience is the basis of all science.

Stephenson (1953) has pointed out that most American psychologists have differentiated between experience and behavior. The distinction has been a very nebulous one since it merely contends that there is behavior which is observable on the one hand and experience which is not observable on the other hand. As Stephenson has noted, there must be a beginning or starting point. Any starting point is going to be arbitrary, though the arbitrariness of selection of the starting point will be subjected to a pragmatic evaluation. Psychologists have tended to use experience as the starting point, but the difficulties of experience as the starting point are readily observable in the confusion which has evolved. The starting point for psychology could just as readily be designated as behavior; and that is all. As Mace (1948) writes, "statements about mind or consciousness turn out to be, on analysis, statements about the behavior of material things." Thus, it would appear that experience, on analysis, is synonymous with behavior and vice versa; since we have two words which are used in the same way, one of them may be eliminated. Moreover, operating within the operations which we call private experience, we have no construct. Thus, Boring's problem is readily resolved—no set of operations, no construct. Since Boring has no set of operations to which he applies the term private experience, he has no construct of private experience, and he can proceed to forget about it.

Objections to this approach may be based on the contention that the resolution of the problem is brought about by denial of the problem. This criticism may have some merit, but at the same time the resolution of the problem may be the reciprocal of the creation of the problem. The problem of private experience was created by the philosophical ancestors of psychology and we can resolve it by denying the problem. It is incumbent upon the creators of the problem to specify the problem

so that it is amenable to solution. If the problem cannot be stated in meaningful terms, then it is rather difficult to provide a solution in meaningful terms. Watson was sorely criticized for denying sensation. Essentially Watson's position was that unless sensation is defined, it is rather difficult to account for it. Psychology has previously presented many amazing paradoxes. James (1884) provided us with a theory of emotion in the absence of a definition of emotion. It may be argued that a definition of emotion is inherent in his theory. However, it would seem that it would be much more fruitful to provide explicit definitions of the constructs for which one is attempting to develop a theory. An additional drawback to inherent definitions is the variability of definitions that may be teased out by the various readers.

### ANOTHER RESOLUTION OF THE PROBLEM

However, there may be another solution of the problem, one which has been suggested by Brunswick (1952). In discussing the subjective-objective conception of data, he suggested that objectivity may be defined in terms of intra-observer or inter-observer reliability of report. In other words, in situations where there is high reliability among the reports of one observer or several observers, there is objectivity. When the reliability of report is low, there is subjectivity. It is here proposed that instead of making our evaluation in terms of a reliability coefficient, we should use probability of obtaining the same verbal statement from one observer on repeated occasions or many observers on one occasion. Thus, the probability of getting the same verbal report (response) from observers looking at a meter is much greater than the probability of getting the same verbal response from observers who are looking at children playing in a nursery school. Much of the advantage of the first type of verbal response rests with the measuring instruments used, but there is also an important element of training involved. The rules of numbers are standard and the numbers are used in the same way in many different countries. Physicists, regardless of the institution at which they were trained, will learn to report the numbers in the same way. In elementary biology classes it is necessary to train students to use a microscope and report events in a similar way. The psychologists, on the other hand, report events in a way which reflects the theoretical biases of their favorite graduate school instructors. Research from the area of verbal conditioning (Krasner, 1958; Salzinger, 1959; Greenspoon, 1961) suggests that verbal behavior is amenable to modification and manipulation in a manner similar to nonverbal behavior. Thus, it may be possible that graduate instructors may reinforce certain verbal responses as descriptive of events and other instructors may reinforce a different set of verbal responses as descriptive of the same events. There is no common data language to describe the events and situations with which the psychologist works. Davis (1953) proposed a physical psychology in which the language of description of psychologists would be compatible with



the language used by other scientists. If Davis means by a physical psychology language a language of the spatio-temporal, then his position may provide us with another means of coping with the problem of private experience. Davis contends that physical psychology will eliminate the concepts of emotion, motivation, etc. It may also eliminate the concept of private experience. However, combining the rules of physical psychology with the probability of eliciting the same verbal response from observers would enable us to measure, and hence operationally define, private experience. If the probability of obtaining the same verbal response from a number of observers on a number of similar occasions approaches zero, we are dealing with private experience. If the probability of obtaining the same verbal response approaches 1.00, we are dealing with public data. Physical psychology would tend to provide a method of making private experience public. By using physical psychology methods, we could introduce a rigor into the data language of psychology which would result in an increase in probability of eliciting the same verbal response from a number of observers. It would enable students to learn a common data language irrespective of the institution of training. It should be recognized that one of the major problems of being a psychologist is that not even a psychologist can escape his behavioral history merely by being a psychologist. In our culture we are not ordinarily trained to report observations in an unambiguous language, but rather we learn to report our interpretations of behavior. Thus, we report that someone is happy or sad, angry or joyous, frustrated or well adjusted, etc. It is rather difficult for students to learn to report events in a language that is relatively free from the observers interpretations of the events. By using a common language of observation psychologists would be able to approach the level of agreement attained in other scientific areas. Physical psychology would either enable events which are now undefined but subsumed under private experience to have a high probability of being reported by many observers or be dropped from the vocabulary of psychology. This suggestion in no way contends that such events are or ever have been psychical. This suggestion does imply that introspective reports should be treated as verbal responses and that is all. The conditions under which various verbal responses are made could be investigated. The trained observers of structural psychology provided verbal responses which had a high probability of being the same. These observers were trained to make various verbal responses under various conditions and they did it very well. Instead of accepting the verbal response as a verbal response, the structuralist considered these responses to describe the content of the mind. Though methodological weaknesses were considered to be the downfall of structuralism, the major difficulty with structuralism, as Stephenson has indicated, was that the structuralists were trying to do something that may be impossible, understanding the workings of the mind. Psychology has not known a more rigorous group of experimenters than the

structuralists, and it is unfortunate that much of their research is ignored today. The present analysis would suggest that the research of the structuralists could be re-analyzed in terms of the effect of instructions and stimulus conditions on the occurrence of various kinds of verbal responses.

It is also possible that this suggested analysis may facilitate the rapprochement of general-experimental psychology and the more applied areas of psychology, e.g., clinical psychology. Historically, clinical psychology developed in an aura of mysticism. Even today the clinical and general-experimental psychologists find it difficult to discuss their respective areas with each other. This situation makes it increasingly difficult to apply both the methodology and findings of general-experimental psychology to clinical practices. Both of these groups would agree that they are studying behavior, but from this point there is a marked divergence in their approaches. A common data language which reflects a high probability of eliciting the same verbal response from a group of observers would provide the two groups with a common meeting ground. Thus, the research from general-experimental psychology could be applied directly to clinical problems.

The foregoing analysis suggests that the concept of private experience, as defined in this paper, would disappear from the literature of psychology. A physical language for psychology may enable psychologists to be trained so that the probability of agreement of verbal report would tend to approach 1.00. Under such conditions the concept of subjective experience or private experience would have no value.

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## WHITTAKER'S "POSTULATES OF IMPOTENCE" AND THEORY IN PSYCHOLOGY

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### THEORY IN PSYCHOLOGY

The necessity for theory in psychology seems generally accepted. However, psychologists do not agree on the position and function that theory should have. One view of the position and function emphasizes the summarizing, after-the-fact explanatory nature of theory. Skinner (1950), and Lee (1950) seem representative of this view. Theory here is an aid in consolidation, designed to enable us better to handle facts which stand on their experimental bases and whose "existence" but not relationship are independent of theory. Another point of view, that of Hebb (1958, 1959), and Dallenbach (1953), seems to emphasize the heuristic value of theory. This point of view, like the summary view, does not question the existing character of fact, but for these theorists, in addition to summarizing the results of experiments, theory is necessary to initiate, guide, and maintain experimentation. Thirdly, a formalistic view of theory exemplified by Spence (1944), Koch (1941), and Bergmann & Spence (1941) considers theory not only as a revelatory procedure, but also as a necessary creative preliminary to thought about experimentation. This point of view differs from the heuristic view both in the broader and more comprehensive role of theory in the design of experiments and in the insistence that empirical statements become facts only within a theoretical framework.

If we may speak metaphorically, the summary point of view is neither tactical nor strategic in nature, being concerned with mopping up operations; the heuristic point of view is tactical; and the formalistic point of view is both strategic and tactical, but, as it has developed, can make only tactical maneuvers because of its unquestioning emphasis on operational, physicalistic constructs.

In the recent past the formalistic view seems to have been dominant in the "Age of Theory" in psychology (Koch, 1959, v. 3, pp. 731ff). The American Psychological Association's seven volume *Psychology: A Study of a Science*, edited by Sigmund Koch (1959), represents in conception the culmination of a formalistic view. However, it is our opinion that, as Parkinson (1957) would have predicted and as Koch in his Epilogue to Volume 3 seems to be saying, this edifice is being constructed at a time when psychology is changing in its emphasis away from formalism. Hebb (1958, 1959), Krech (1949, 1950a, 1950b), and

others have decried the sterility of such an approach and are advocating the point of view which we have termed heuristic.

It is interesting to note that these advocates of the heuristic position, notably Hebb, offer arguments similar to those of Beck (1950) for inferred entities. While the countearguments of Noyes (1957) seem closer to our particular mode of thinking, we do not here intend to enter directly into arguments concerning inferred entities versus mathematical structures or realism versus nominalism or subjectivism (Eddington, 1939; Zellingner, 1956; George, 1953). Rather, we wish to present a point of view which we feel is logically prior to the above arguments. We present it with the feeling that either acceptance or rejection of it can sharpen and carry forward our understanding of theory in psychology.

#### WHITTAKER'S POSTULATES OF IMPOTENCE

Whittaker (1949, 1952) in his discussion of the development of physical science has called attention to "postulates of impotence" which assert "the impossibility of achieving something, even though there may be an infinite number of ways of trying to achieve it" (1949, p. 59).

"These are not hypotheses of a positive nature about the structure of of world around us, but statements of a negative character, to the effect that something is impossible . . . In the philosophy of science, a postulate of impotence occupies a peculiar position. It is not a direct inference from experiment, such as is met with in experimental physics; nor is it, like the theorems of pure mathematics, a necessary consequence of the structure of the human mind; nor is it again, like most of the hypotheses of theoretical physics, a creation of the free intellect: it is simply the statement of a conviction that all attempts to do a certain thing, however made, are bound to fail. The postulates of this type already known have proved so fertile in yielding positive results—indeed a very large part of modern physics can be deduced from them—that it is not unreasonable to look forward to a time when the entire science can be deduced by syllogistic reasoning from postulates of impotence" (1952, pp. 52-53).

These postulates of impotence, by forming a base beyond which the science cannot hope to effect any experimentation, allow of changes of emphasis and direction in research. It appears to us that there are possible in psychology today two basic postulates of impotence, the tentative acceptance of which could further the field.

#### *Postulate of Conceptual Impotence*

The first such postulate we have termed the *postulate of conceptual impotence*. If we follow Whorf (1956) in saying "that it is not possible to define 'event, thing, object, relationship,' and so on, from nature, but that to define them always involves a circuitous return to the grammatical categories of the definer's language" (p. 215), then not only is language the arbiter as to the units of behavior chosen for research, but

also we are impotent in our attempts to search beyond the definitions given by our language.

In this sentence Whorf, to repeat Whittaker, asserts "the impossibility of achieving something even though there may be an infinite number of ways of trying to achieve it." While Whorf presented this as a statement of linguistic relativity, it is not certain that he fully accepted the impotence implied by it, therefore we are not calling on Whorf to justify the use to which we put his ideas. It is our contention that he found it necessary to speak elsewhere of language as a "superficial embroidery upon deeper processes of consciousness" (p. 239) because he failed to accept the postulate of impotence contained in the sentence previously quoted. His attempt to deny impotence led him to specify conditions (similar or calibrated linguistic backgrounds) which would do away with his "new principle of relativity" (p. 214). It is our further contention that the lack of such a postulate of impotence renders Whorf liable to Black's criticism that his theory suffers from "the familiar paradox that all general theories of the relativity of truth must brand themselves as biased and erroneous" (Black, 1959, p. 237).<sup>1</sup> Formulations similar to Whorf's hypothesis such as those of Sapir, Wittgenstein, Cassirer, et al., also may be seen as not carrying the implication to the points of impotence which have already "proved so fertile in yielding positive results" in other fields.

If we do accept as a postulate "that it is not possible to define . . . from nature," one must first recognize the arbitrary nature of our psychological terms and secondarily the irreducible uncertainty as to their referents. We have called this uncertainty *conceptual uncertainty*. There seems to us to be a large enough number of conceptual uncertainties in psychology to justify the necessity for an examination of a postulate of conceptual impotence in this field. The number of such uncertainties is too large for presentation here. Rather than present an extensive listing of examples, we have chosen to give one or two examples from several areas. The field of intelligence testing and theory is replete with uncertainties (cf. A. W. Heim, 1954). Howell (1947) and Woodger (1953, 1956) have spoken of the uncertainty of the distinction between inheritance and acquisition of characteristics. Goldstein (1959) has questioned the self-evident nature of fact in neurophysiology. Dewey (1896), early, and Verplanck (1954), Miller (1959), and Chomsky (1959), recently, have underlined the basic uncertainty as to what response and stimulus refer to. Peters (1958) speaking of motivation points out the impossibility of developing a general theory of motivation since the way in which the question is asked enters into the answer. Kendler (1952), in presenting "what is learned" as a pseudoproblem, started a controversy (Smedslund, 1953; Ritchie, 1953; Campbell, 1954; et al.) which indicates the uncertainty underlying

<sup>1</sup>By our presentation of the Whorf hypothesis as a postulate of impotence, it is, in effect, removed from another, more usual area of criticism, namely its testability. (Cf. Greenberg, 1954; Lenneberg, 1953; Whatmough, 1956, for such criticism.)

the word "learned." Kelly (1955) with his postulate of "constructive alternativism" not only stresses the arbitrary nature of our personality constructs, but exemplifies what we feel Whittaker means by the "fertile" use of such postulates. Rommetveit (1954) in his treatment of "social norms and roles" provides a similar example for social psychological concepts. In the field of troublesome behaviors Cameron (1953) indicates the implicit assumptions underlying the theory of diagnosis which lead to irreducible uncertainty as to "cause." Lenz (1956) spells out our necessary indecision as to whether probability should be viewed as logical or empirical or intermixed. In linguistics Preston (1947, 1948a, 1948b, 1949) and Chomsky (1957) point out the dependence of the results of linguistic analysis on the methods used by the linguist and the consequent impossibility of determining the only or even the optimum structural analysis for a particular language.

Diverse though the articles cited above may be in content and intent, they are similar in that they point out the difficulties one faces in attempting to specify the referents of our technical terms. Following Whittaker one might argue that a recognition and frank statement of these uncertainties should lead us to state as a postulate of impotence that we can deal with nothing other than linguistically determined units. It seems to us that this postulate of impotence, by forming a base beyond which psychology can not hope to effect any experimentation, allows of changes of emphasis and direction in psychological research. In the first place the danger of reification is underlined, but more important it will be recognized that in the isolation of the conceptual unit one has, to some degree, determined a class of explanatory concepts which will emerge in investigation. Such recognition is necessary to bring an end to the search for unconfounded essences (e.g., attempts to specify the nature of man or of intelligence) and to shift the emphasis from concepts of parameters of mankind to concepts as to why man behaves in this conceptualized way in this conceptualized situation. It is our further contention that this not only enables us but forces us to recognize the deductive nature of scientific knowledge and enables us better to understand the nature and meaning of our inductive generalizations. That it destroys our hopes of coming to grips with any hypostatized "real world" is implied in the use of the word "impotence." One can not speak of successive approximations to "reality" unless one has prior knowledge of the position of the target. Accuracy gives way to precision and validity to internal consistency.

#### *Postulate of Operational Impotence*

In a sense the preceding argument would make conceptual impotence prior to and basic to our second postulate which we have called the *postulate of operational impotence* (cf. Hutten, 1954, p. 163-4). Following the Heisenberg principle of uncertainty one may argue that whatever the conceptual units, or whatever the relation between these

units and a real world, the act of investigation renders imperfect our specifications of the results (cf. Whittaker, 1952, p. 143). This is to say that not only does the relative nature of our concepts render us impotent, but that the tools of investigation, be they observation, experimentation, or instrumentation, also are confounding our indices of a real world. The uncertainties resulting from this confounding we have termed *operational uncertainties*, both to describe the "source" of the uncertainty and to contrast this source with the conceptual source.

It is not our purpose to consider the place and meaning of Heisenberg's principle of uncertainty in the physical sciences (cf. Bohr, 1958; Einstein, 1949; Hanson, 1959). Rather, the kind of uncertainty resulting from our interference which we have reference to is that indicated by Harlow, speaking from a psychological or behavioral point of regard, when he said "Bilateral removal of the temporal lobe in the monkey produces a new species or subspecies of primate that behaves differently on our tests from all or most other monkey species!" (Harlow 1958, p. 5) It seems to us that even where our conceptual units are exact, such operations (double meaning intended) as these render imperfect our specifications of the species with which we are dealing.

Other operational uncertainties of this sort which justify the examination of the postulate of operational impotence can be found in the literature. As previously we shall list no more than a few. An operation less destructive than the one Harlow refers to but one equally capable of rendering our specifications of species imperfect is illustrated in Benoit's work with DNA (Benoit, Leroy, Vendrely, & Vendrely, R., 1957; 1958; 1959). Parenthetically, what this work with DNA does to the concept of the gene as operationally defined is still being argued (cf. McElroy & Glass, 1957). Krech (1959) in discussing underfeeding as a technique for motivating asks if "we have two quite different brains involved" (p. 8) when comparing hungry with sated rats. Taylor and Birmingham (1959), dealing with man-machine system performance, indicate how easily system behavior can confound our indices of human behavior. A further illustration of a confounded system performance in which our experimental methods rather than a machine renders our operations uncertain is that of Goldiamond (1958) and Goldiamond & Hawkins (1958) on subliminal perception. Brunswik's (1947) many illustrations of the confounding introduced by the "artificial tie" set up between variables in systematic designs of experiments are also relevant here. Jeffrey (1956) in discussing the operational nature of probability considerations pointed out "both the Bayes and minimax criteria permit choice between hypotheses only with respect to a set of utilities which in turn are relative to the intended applications of the hypotheses" (p. 245). This list of examples in which our operations have rendered imperfect our specifications of the results could be added to or even multiplied. It is felt, however, that these few are sufficient to make our point.



A word should be said regarding "uncertainty" or "imperfect specification." These terms have meaning only insofar as they relate our knowledge to an hypostatized real world. To the extent that we accept them, we accept our impotence to make statements about a real world independently of interaction with measuring instruments. It is our contention that a recognition of the postulate of operational impotence will supplement our knowledge in two ways. In the first place, paraphrasing Bohr (1958, p. 26), information regarding the behavior of subjects obtained under definite experimental conditions may be characterized as complementary to any information about the same or similar subjects obtained by some other experimental arrangements.<sup>2</sup> Brunswik's (1947) use of "ecological validity" to resolve apparent contradictions between data obtained under varying conditions seems a good example of supplementation in this sense. However, even more basic than this, recognition that no experimental result can be interpreted as giving information about independent properties of the objects of inquiry should lead us to see that methods and what methods contribute to results are as much objects of inquiry as the data they provide. Taylor and Birmingham's (1959) article is especially apropos here. In a sense what we are saying is that not only will the contribution of the operation to the system be recognized, but the inextricable embedding of the behavior in the system will lead us never to speak of "behavior" of the organism. This appears to us to be other than an emphasis on the situational character of behavior; it is rather saying that we "know" behavior only as conceptually determined, operationally determined emergences. The attempt to extricate the operator from the system is fruitless, but to recognize "the operation, the organism, and the behavior" as the basic unit of investigation is mandatory for understanding the meaning of any inductively-arrived-at generalization. (Cf. Krech's suggestion that psychology should re-examine its units of analysis [1959, p. 8].)

#### THE POSTULATES AND A LOGICAL MODEL OF SCIENTIFIC THOUGHT

It is our argument that the two postulates of impotence we have considered here are necessary postulates within what Schmidt (1957) termed the logical model of scientific thought. After having traced the history of the geometrical model and the physical model, Schmidt concludes that our present model is best term "logical."

"We might summarize these historical developments leading to the third model of scientific thought in the following theses:

- (1) The quest for certainty is dead, knowledge is probable.
- (2) Mathematical propositions tell us nothing about the character of nature; they are uninterpreted formalisms.
- (3) Experience is the sole test for knowledge.
- (4) Deductive pro-

<sup>2</sup>It seems to us that starting on page 27 of this article, Bohr gradually shifts his emphasis from epistemological questions to (on page 28ff) metaphysical statements. It seems still further that contemporary discussions concerning complementarity also emphasize metaphysical implications to the detriment of epistemological questioning.

cedures cannot test the truth or falsity of scientific knowledge but are necessary tools for the elaboration of its consequences. (5) There exist alternative sets of mathematical axioms and physical laws among which we may choose *arbitrarily* to form scientific theories so long as this combined whole leads to certain empirical consequences. The existence of such alternative components means that questions concerning whether nature is really this way or that are factually meaningless because experience in principle can render no decision. (6) Scientific knowledge organizes experience and does not describe some real nature. (7) It is systematic, thus reliable for decisions, and sufficient for our needs and purposes. (8) It is *objective* among observers for communication and public scrutiny; and *relative* to a public frame of reference. Such is our conception of scientific thought after 25 hundred years of development and clarification, but I do not think this is the end of its history" (p. 149).

It seems to us that this is a pertinent and useful description of scientific thought. It should be noted, however, that if the implications of characters three and six taken together are carried out, we must conclude that our scientific concepts and methods are our sole tests for knowledge. Since scientific knowledge is systematic, reliable for decisions, and sufficient for our needs, this circularity between experience as the test for knowledge and experience as organized by knowledge might lead us to undue acceptance of our current concepts and methods, hence our knowledge, as final. It is in such a context that we feel an explication of the two postulates of impotence is crucial, not only to guard against the assumption of finality, but by forcing us to examine our concepts and methods and their interrelations to lead to new orderings of experience. It is also in such a context that we find ourselves in agreement with Meissner's (1960) attitude toward attempts to develop a "methodologically complete behaviorism," namely, "that we cannot afford to rest easy with this sort of methodological act of faith" p. 69). Meissner concludes that many questions "will find their solution, not only in and through the analysis of theory and metatheory, but on the level of the philosophic presuppositions that are active in almost every phase of the discussion" (p. 69). It is just such a treatment we have attempted in the present article.

#### SOME IMPLICATIONS OF ACCEPTANCE OR REJECTION OF THE POSTULATES

We mentioned earlier that we felt we could "sharpen and carry forward our understanding of theory in psychology" by either acceptance or rejection of the postulates of impotence. It is beyond the scope of this paper to discuss fully the implications of this for any theory of knowledge, but one implication must be considered. It seems to us that acceptance of the postulates leads to uncertainty about the unknown, whereas rejection of the postulates leads to uncertainty about the known.

To elaborate: if one were to accept the postulates of impotence, questions of validity, as has been mentioned, give way to questions of internal consistency. Since what is known will be seen as created, we may, following Schmidt, use organized experience as a test for the consistency of our system. However, the absence of data of experience (that which is unknown) gives no such check and might thus be either an unknown created by our concepts and operations—hence unknowable under that system, or might be a function of faulty or incomplete logic. The deliberate and self-conscious acceptance of the postulates would require recognition that a given systematic scientific knowledge (conceptual and operational) can preclude investigation in areas of experience unordered by that scientific knowledge. Theory is then seen as a necessary aspect of the denotation (conceptual and operational) of the experiential referents of our scientific knowledge as well as a necessary determinant of our criteria of consistency. Under this view, scientific work receives its impetus from the recognition of inconsistencies within the extant scientific organizations of experience and from the search for new organizations which becomes mandatory when it is held that knowledge is rendered "imperfect" by conceptual and operational impotence.

On the other hand if one were to reject the postulates of impotence, the unknown might be seen as a function of human fallibility (a metaphysical statement of our postulates), of the degree of maturity of the science, or of the essentially mysterious, hence unknowable, aspects of "nature." Disregarding the first as metaphysical, the last as theological, we see the middle alternative as the view held by most scientists. The unknown, thus, does not lead to uncertainty, but is, in science, a stimulus to research. The known, however, does lead to uncertainty about the accuracy, or validity, or truth, of our scientific knowledge. The deliberate and self-conscious rejection of the postulates must inevitably lead to the recognition that the datum becomes a datum not through the "fact" of its having been gathered but through the "fact" of its meeting criteria of accuracy, validity, or truth. To the extent that these criteria are unspecified, then we need not worry about the place of theory in psychology (or any science). But to the extent that we wish to know the foundations of our data, we must specify our criteria. Theory then must be seen as an integral part of the search for demonstrations of accuracy, validity, or truth of any scientific knowledge.<sup>3</sup>

### SUMMARY

In understanding the place of theory in psychology, it is strategically important for psychologists to accept or reject two "postulates of impotence" (Whittaker). The postulate of conceptual impotence forces us to recognize the arbitrary nature of our psychological terms and the

<sup>3</sup>It is our impression that the contributions that "construct validity" has made would be more powerful had they arisen from a considered rejection of the postulates of impotence.

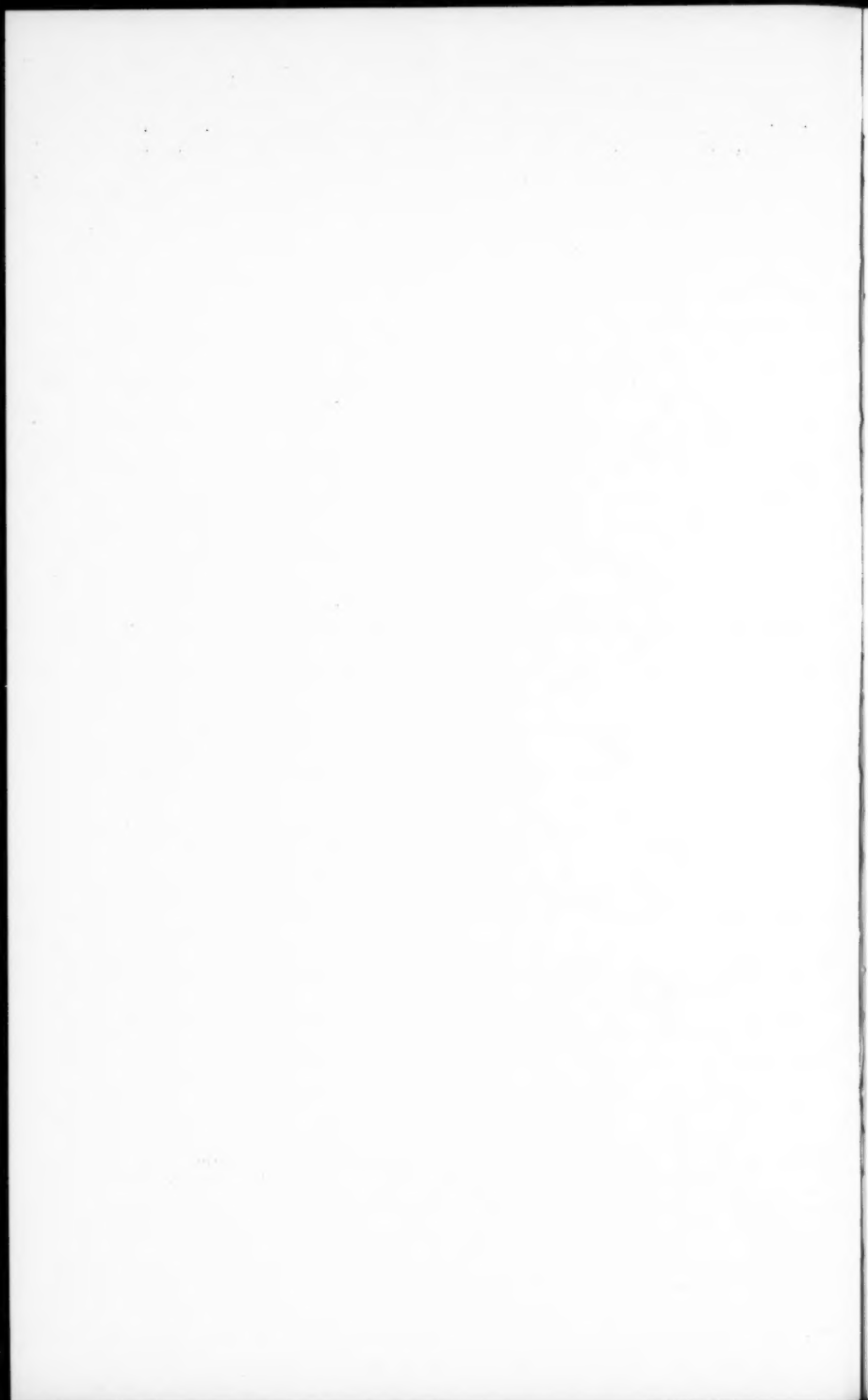
irreducible uncertainly as to their referents. The postulate of operational impotence maintains that not only does the relative nature of our concepts render us impotent, but that the tools of investigation, be they observation, experimentation, or instrumentation, also are confounding our indices of a real world. Consideration of these is necessary in the "logical model of scientific thought" (Schmidt).

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## FREQUENCY OF REINFORCEMENT AND PRELIMINARY TRAINING CONDITIONS AS DETERMINANTS OF EXTINCTION<sup>1</sup>

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Conditioned operant behaviour in a lever-pressing apparatus takes the form of a conditioned *heterogeneous* response chain (Skinner, 1938; Frick & Miller, 1951; Spence, 1956; Keller & Schoenfeld, 1950). In earlier papers (Hurwitz, 1958, Millenson & Hurwitz, 1961) we reasoned that the pattern of behaviour systematically reinforced in the apparatus involves moving towards the lever, pressing the lever (L), moving towards the food tray (T), retrieving the food and eating. By sampling more than one member of such a response-chain, the more commonly used measures of conditioned behaviour like response rate, resistance to extinction, per cent correct choice, latency between signal and response, can be extended to include serial dependencies between individual component members. In the present study a relative frequency

measure,  $P_L = \frac{fR_L}{f(R_L + R_T)}$ —where  $fR_L$  and  $fR_T$  refer to the total number of lever responses respectively, was used in preference to the serial dependency measures of  $P_{L/T}$  and  $P_{T/L}$ , i.e., the probability of observing a lever response following a tray response, and the probability of observing a tray response following a lever response, respectively. (See Cane, 1956, Hurwitz, 1958.)

It is customary to precede response-chain reinforcement sessions by training sessions which involve the reinforcement of terminal members of the response-chain only. Thus, in the example given above the tray response usually has a longer reinforcement history than other response components. The problem investigated was whether the frequency of reinforcement given to terminal response-chain components is a determinant of performance under experimental extinction, with the number of response-chain reinforcements acting as a parameter.

### METHOD

#### *Subjects*

Thirty-five experimentally naive male hooded rats were used in the experiment. Ss were approximately 3 months old and were drawn from the closed colony of the animal laboratory.

<sup>1</sup> The experiment was subsidized by a grant from the Central Research Fund, University of London, 1955.

### Apparatus

The lever-pressing apparatus was mounted in a sound-proofed chamber. It measured 12 in. x 9 in. x 12 in. and was fitted with a food delivery mechanism which dispensed .05 gm. pellets into a food tray. The lever and food tray were positioned alongside one wall of the apparatus. The lever measured  $\frac{1}{2}$  in. sq. and responded to a dead weight of 5 gm. It was placed 5 in. to the right of the food tray. Pressing the lever activated the food dispenser through a relay circuit and sounded a buzzer, placed 12 in. away from the apparatus. The food tray was shielded by a light opaque door which had to be nuzzled open before food could be retrieved. The relay circuit was wired so as to deliver only 1 pellet at a time and made it impossible for S to accumulate a heap of food by repeatedly pressing the lever without also opening the tray door.

Ss could be observed through a window cut into the soundproofed chamber.

Response frequencies were recorded on counters.

### Procedure

Table 1 summarizes the design of the experiment. There were six experimental groups which differed from each other in terms of the number of tray-response reinforcements during preliminary training sessions and the number of response-chain reinforcements during the second phase of the experiment, as shown in columns III and IV respectively.

Two weeks before the experiment proper was run, Ss were put on a 22-hour food-deprivation schedule. This schedule was maintained

TABLE 1

DESIGN OF EXPERIMENT, MEDIAN NUMBER OF LEVER AND TRAY RESPONSES DURING EXPERIMENTAL EXTINCTION, AND MEAN  $P_L$ , WHERE  $P_L = \Sigma R_L / \Sigma R_L + \Sigma R_T$

Group	Number of Subjects	Reinforcement Scores		Extinction Scores		
		III Reinforced $R_T$ 's	IV Reinforced response chains	V $R_T$	$R_L$	VI $P_L$
A	6	10	30	89	26	.216
B	5	30	30	133	101	.418
C	9	90	30	133	70	.336
D	5	30	90	90	101	.540
E	5	90	90	194	131	.387
F	5	90	150	78	147	.662

TABLE 2

Group	Subject	Extinction Scores		
		R <sub>T</sub>	R <sub>L</sub>	P <sub>L</sub>
A	1	109	40	.27
	2	89	18	.17
	3	89	32	.26
	4	210	26	.11
	5	88	26	.23
	6	83	21	.20
B	1	95	60	.38
	2	133	72	.35
	3	99	101	.51
	4	144	112	.44
	5	165	112	.41
C	1	130	99	.41
	2	161	83	.34
	3	166	99	.37
	4	100	66	.40
	5	133	71	.35
	6	165	66	.29
	7	103	67	.38
	8	211	73	.26
	9	122	70	.35
D	1	90	98	.52
	2	104	110	.54
	3	85	81	.48
	4	102	104	.50
	5	83	101	.54
E	1	133	72	.35
	2	205	139	.30
	3	207	142	.42
	4	150	89	.36
	5	194	131	.40
F	1	97	147	.60
	2	40	165	.80
	3	72	190	.72
	4	78	143	.64
	5	108	139	.56

throughout the experiment, each S being run half an hour before feeding time.

(a) *Preliminary, tray-training trials* ( $S^D: R^T \rightarrow S^R$ ) Each S was given at least 10 tray reinforcements in addition to the 3 pellets placed

into the tray on the first day of the experiment. The 10 reinforcements were administered aperiodically, at mean intervals of 20 secs. Group A received 10 pellets, Group B and Group D 30 pellets spread over three separate sessions, and Group C, Group E and Group F received 90 pellets spread over three sessions. The presentation of each pellet was accompanied by a buzz of .5 sec. duration.

(b) *Response-chain, conditioning trials* ( $R_L \rightarrow S^D : R_T \rightarrow S^R$ ) Following tray conditioning trials, Ss were placed into the apparatus and removed only after 30 pellets had been obtained by first pressing the lever and then responding to the tray. Group D, Group E and Group F received a further training session the following day. This session terminated when 60 pellets had been obtained. Group F was given a further session the following day (60 pellets).

(c) *Experimental Extinction* The final phase of the experiment involved a two-hour experimental extinction session. The food dispenser was emptied except for 3 pellets. The buzzer was left connected to the lever.

## RESULTS

Table 1 summarizes the results in terms of median  $R_T$ 's, median  $R_L$ 's and the measure  $P_L$ .  $P_L$  is defined as the relative frequency of lever responses to the total number of lever and tray responses, i.e.,  $P_L = \frac{fR_L}{f(R_L + R_T)}$ . Table 2 gives a subject by subject breakdown

of the extinction data. Since the assumption of homogeneity of variance seems unwarranted, non-parametric analyses of variance by the Kruskal-Wallis method (Siegel, 1956) were performed. The first group analysis involved lever-frequency scores drawn from Group C, Group E and Group F. These groups had all been given an equal number of tray reinforcements during preliminary training sessions, but differed in terms of frequency of response-chain reinforcements. The group means, Group C = 70, Group E = 131 and Group F = 147, were found to differ significantly ( $p = .05$ ). A similar analysis of tray response frequency during experimental extinction showed group differences to be significant at the .01 per cent level; the mean tray scores being Group C = 133, Group E = 194 and Group F = 78.

In general, these results indicate that the rate of  $R_L$  increases and the rate of  $R_T$  decreases during an extinction session as a function of the frequency of response-chain reinforcement for certain values of the tray-training variable. These relationships are more fully represented in Figures 1 and 2.

Figure 1 is divided into two sections. The upper section represents the median number of lever responses ( $R_L$ ) during an experimental extinction session as a function of the frequency of response-chain reinforcements ( $R_L \rightarrow S^D : R_T \rightarrow S^R$ ). In the lower section the dependent

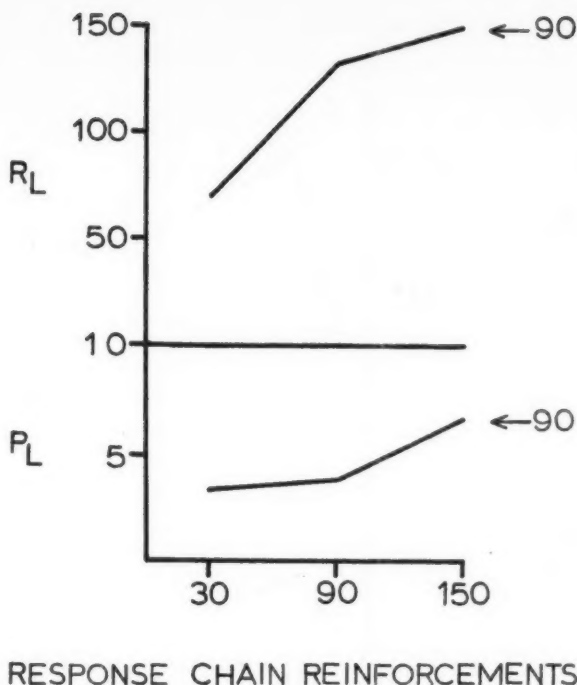
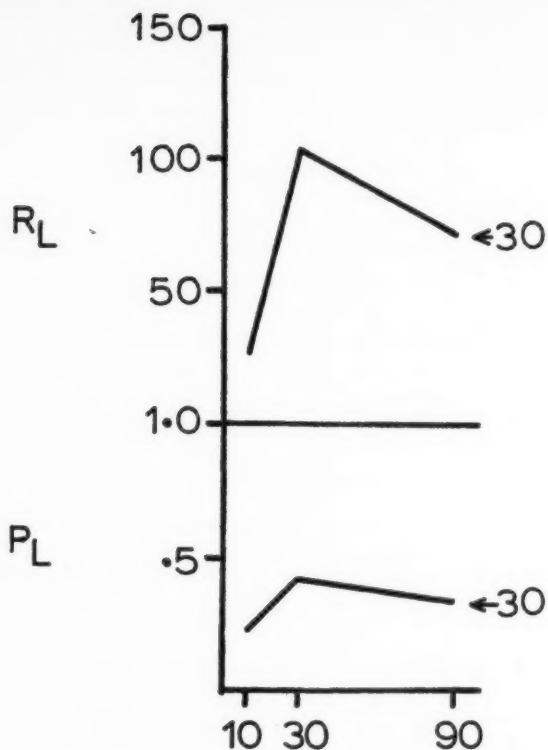


Figure 1. Lever response frequency (upper section) and relative frequency of the lever response (lower section) as a function of the number of response-chain reinforcements. The parameter, frequency of tray contact reinforcements during preliminary training trials, is set at 90.

variable is the relative frequency measure,  $P_L$ . These two functions have frequency of tray reinforcements during preliminary training sessions ( $R_T \rightarrow S^R = 90$ ) as a parameter. Figure 2 is likewise divided into two sections. The measures  $R_L$  and  $P_L$  are represented as functions of the frequency of tray reinforcements during preliminary training, with the frequency of response-chain reinforcements as a parameter ( $R_L \rightarrow S^D : R_T \rightarrow S^R = 30$ ).

Figure 1 is interpreted as showing that the frequency of lever responses exceeds the frequency of tray responses as a function of the number of response-chain reinforcements, when  $R_T \rightarrow S^R = 90$ . This hypothesis was tested and supported by an analysis of variance on the relative frequency measure  $P_L$  ( $t = .01$ ), the major difference being between Groups C and E vs. Group F.

The data represented in Figure 2, which includes scores from Groups A, B and C, was similarly submitted to a series of significance tests. These groups differed from one another in terms of the number of tray reinforcements received during preliminary training sessions but were equated for the number of response-chain reinforcements (30).



### TRAY REINFORCEMENTS

Figure 2. Lever response frequency (upper section) and relative frequency of lever response (lower section) as a function of the frequency of tray contacts during preliminary training sessions. The parameter, frequency of response-chain reinforcements, is set at 30.

The difference in the number of lever responses during experimental extinction (Group A=26, Group B=101, Group C=70) was significant at the .001 level, and the relative frequency measure,  $P_L$ , also distinguished these groups ( $p = .01$ ).

Lastly, the extinction performances of Group B and Group D were contrasted. These groups were equated on the number of tray reinforcements received and differed with respect to the number of response-chain reinforcements (30 and 90 respectively). A Mann-Whitney U test was used on the  $P_L$  values and yielded a  $p = .02$ . This result is noteworthy since these groups are not distinguishable in terms of either  $R_L$  or  $R_T$  alone.

## DISCUSSION

Two conclusions may be drawn from these results. Firstly, the amount of preliminary training involving reinforcement of tray-approach responses in a lever-pressing apparatus may act as one of the determinants of response frequency during extinction. Secondly, the number of response-chain reinforcements may noticeably affect measures of response rate and of relative response frequency.

*Interpretation*

The acquisition of the response-chain described in the introduction involves at least two discriminations: (i) approaching the tray following presentation of the discriminative stimulus,  $S^D$ ; represented by ' $S^D: R_T \rightarrow S^R$ ', where the colon indicates that a stimulus has set the occasion for a response and the connecting arrow indicates that the response has produced a stimulus change; (ii) approaching and then pressing the lever after a pellet has been eaten, i.e.  $R_T \rightarrow S^R: R_G \rightarrow s_G: R_L \rightarrow S^D$  where, in addition to the symbols already made familiar,  $R_G$  and  $s_G$  stand for the consummatory eating behaviours and whatever (unobservable) stimulus consequences are produced by such behaviours.

These discriminations may be simultaneously or successively acquired. Under the experimental procedure used, the discrimination is of the successive kind. Previous work had shown one requirement of the response-chain is very rapidly learned, namely the discrimination ' $S^D: R_T \rightarrow S^R$ ' (Hurwitz, 1958; see also Millenson & Hurwitz, 1961). Measured by the conditional probability  $P(T/L)$  its asymptote is reached within as few as 25 reinforcements. However, the second discrimination, which involves the recycling of the response-chain, i.e.  $R_G \rightarrow s_G: R_L \rightarrow S^D$  and measured by the conditional probability  $P(L/T)$ , requires at least 60 reinforcements to reach the .5 value. Often more than a hundred further reinforcements are needed to reach the asymptote (see Hurwitz, Brener & Jones, 1961). Thus, the tray response is more difficult to bring under discriminative control than the lever response, and this may be explained by the following four considerations. Firstly, the tray response has a higher operant level than the lever response, i.e., occurs with a higher frequency before reinforcement operations are applied to it; secondly, from the very first the tray response is submitted to an intermittent reinforcement schedule which tends to increase its probability of occurrence (Jenkins & Stanley, 1950); thirdly, the tray response is closer to the point of reinforcement than the lever response. According to the gradient of reinforcement principle (Hull, 1943, Spence, 1956), this strengthens such a response more than earlier responses in the chain; finally, the discriminative stimulus for the lever response is ill-defined and ambiguous and thus does not provide the appropriate conditions for eliciting competing responses to tray approaches (Estes, 1959). The problem therefore seems to be to provide a



sufficiently distinctive cue at the tray to indicate alternatively when reinforcement is available and when the emission of a tray response will go unreinforced. Ideally such a stimulus should terminate after the response elicited by reinforcement is completed. Experiments to investigate this problem are in progress. In these experiments the tray is illuminated whenever reinforcement is available and is plunged into darkness as soon as the pellet has been retrieved. By this technique the control of the lever-pressing response is removed from cues arising from the animal's own behaviour (namely those arising from eating and indicated by  $s_g$  above) and comes directly under experimental control.

Under the ambiguous  $S^D$  conditions of the present experiment it might be expected that the exclusive control of  $R_T$  will only slowly pass to the relevant  $S^D$ 's, so that in their absence tray responses will not be emitted. Under experimental extinction, this would be revealed by fewer  $R_T$  responses and more  $R_L$  responses. The results support this prediction. When the amount of preliminary training is equated, as in the case of Groups C, E, and F, where 90 tray reinforcements were administered, and Groups B and D, where 30 tray reinforcements were administered, and the number of response-chain reinforcements is varied, the measure  $P_L$  varies systematically. The greater the number of response-chain reinforcements, the larger the value of  $P_L$  (Group C,  $P_L=.336$ ; Group E,  $P_L=.387$  and Group F,  $P_L=.662$ ; Group B,  $P_L=.418$ ; Group D,  $P_L=.540$ ).

Collateral support for this interpretation comes from an experiment in which the training procedure ensured that, in the case of one group of subjects, the control of the tray response had passed to stimuli produced by the lever response (Hurwitz, 1962). The procedure is known as a counting schedule (Millenson, 1961). A fixed number of lever responses has to occur before a tray response is reinforced. A second group was trained by the classical fixed ratio procedure. Here too, reinforcement occurred after a fixed number of lever responses had occurred, but no restriction was imposed on the serial order of response emission. Thus, any array of lever and tray responses resulted in reinforcement if the requisite number of lever responses formed part of the sequence. During experimental extinction  $S_s$  trained by the counting procedure yielded a  $P_L=.69$  and  $S_s$  trained by the fixed ratio procedure yielded a  $P_L=.43$ . The absolute frequency measures, too, lay in the direction predicted by the present analysis in terms of discrimination learning factors:  $S_s$  trained by the counting procedure, where the tray response comes under strong discriminative control, made twice as many lever responses and half as many tray responses as  $S_s$  trained by the fixed ratio procedure.

Thus we conclude (i) that the reinforcement history of operant responses which enter into a response-chain is a factor determining performance during experimental extinction. Since frequency of response under experimental extinction has been amongst the most commonly

used indices of response strength, the isolation of its determinants is of theoretical interest (Hull, 1943, Spence, 1956); (ii) although the lever-pressing apparatus is being increasingly used for isolating behavioural variables and investigating behavioural principles using mice, rats, monkeys and human subjects, many of the parameters of performance in this experimental situation, especially discrimination factors, have received insufficient examination. The present study has demonstrated that the acquisition and extinction of response-chains may be viewed in terms of discrimination learning factors, without straying too far from the empirical touch-line.

### SUMMARY

The correlation of two variables, the number of reinforcements during preliminary training sessions and the number of reinforcements during lever-pressing training sessions, with a measure of response-chain conditioning, was investigated. Six groups of male rats (35 Ss) were trained in a lever-pressing apparatus, food serving as reinforcement. Two responses were recorded (a) a lever-pressing response; (b) a tray-contact response. Preliminary training sessions consisted in reinforcing tray-contact responses only; lever-pressing training consisted in presenting the reinforcer after a lever press was followed by a tray response. A subsequent extinction session showed that the functional relation between extinction score and frequency of reinforcement may be influenced by the amount of preliminary reinforcement given.

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## TOWARD A UNIFIED PSYCHOLOGY

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Since the advent of modern empirical psychology, the metascientific framework appropriate to the physical sciences has been insistently promoted as eminently suitable for the containment and ordering of psychology's subject-matter. That psychology may be a unique science which must evolve a metascience addressed to the many problems peculiar to itself—and not encountered in physical science—is a possibility rarely entertained.

Without evidence, it is maintained by some that the psychological subject-matter is not different in kind from that of physical science. Herein, it is asserted that the legitimate subject-matter of psychology has been systematically ignored, semantically circumvented, attenuated, and otherwise sacrificed to an image of science *qua* Science in the very pursuit of a scientific psychology, and that notions having no value beyond their congeniality to a physicalistic metascience continue to be cherished on this ground to the impediment of a coherent psychology.

Being founded in mental operations rather than physical ones, the validity of a particular metascientific structure may be judged by means of logical analysis; representing a formal expression of the special conventions of a scientific discipline, metascience itself is not susceptible to experimental test. The signal success-record of the physical sciences attributed *post hoc* to their metascience (itself outlined *post hoc* by the physicist and philosopher of science) does not constitute empirical evidence that this metascience has a range sufficient to encompass the subject-matter of psychology. However, the sciences-of-status can lend their persuasive authority to the physicalistically-minded psychologist who, assuming the mantle of True Scientist, may take the field by semantic storm.

The present paper will examine several major issues which bear on the possible establishment of a metascience addressed to the problems peculiar to a scientific psychology. Since these issues largely depend on the use of mental operations for their resolution, they are amenable to more than one direction of interpretation. Perhaps *because* they are founded in mental operations such issues have been scanted by psychologists-at-large in favor of the "hard facts" derivable via laboratory procedures, yet the physicalistically-oriented have not failed to grasp their significance at the metascientific level—interpreting them to fit the antecedently established position that the subject-matter of psychology is not different in kind from that of physical science.

## THE PERSISTING ISSUE OF DUALISM . . .

The future historian of matters psychological may pause to wonder at the enormous (and perhaps appallingly wasteful) expenditure of intellect devoted to the eradication of dualism from psychology. If dualism is regarded dispassionately, the urgency will be found to arise not from its intrinsic implications for psychology but from something *extrinsic*—a desire to accommodate the subject-matter of psychology to the metascientific framework of the common run of natural sciences. Seemingly, if psychology cannot be reduced to fit the common mold it must accept a status inferior to that of physical science. The opposite point of view—that psychology may be a unique science whose tremendous reach and extraordinary complexity entitles it to a special status—has long been out of fashion. Even so, the psychology of science and scientists is as much a part of its subject-matter as is the psychology of monkeys, rodents and lesser organisms as these exist within *their* special environments.

William James, an outspoken dualist, says of *cognition*:

*The psychologist's attitude toward cognition . . . is a thoroughgoing dualism. It supposes two elements, mind knowing and thing known, and treats them as irreducible. Neither gets out of itself, or into the other, neither in any way is the other, neither makes the other. They just stand face to face in a common world, and one simply knows, or is known unto, its counterpart. This singular relationship is not to be expressed in any lower terms, or translated into any more intelligible name (James, 1890, vol. I, p.218).*

If this "singular relationship" is considered in all the bluntness of James's statement, it is difficult to discover where psychology might be harmed thereby. Nevertheless, the frequent charges and countercharges of dualism are taken seriously in modern psychology, thus operating in a disjunctive capacity within the field while serving the supposedly larger image of science *qua* Science. If psychology's subject-matter does indeed have a subjective core not to be found in the insensate substance with which the physical scientist is concerned, simple acceptance of the fact should promote harmony within the field—and no amount of semantic exorcism can prevail if dualistic conceptualizations reflect an underlying reality.

Expounding the thesis of physicalism, Stevens (1939, p. 240) sees the "traditional but somewhat antiquated problem of psychophysical dualism" as *exclusively* a problem of syntax, and says:

All sentences purporting to deal with psychical states are translatable into sentences in the physical language. Two distinctly separate languages to describe physics and psychology are therefore not necessary. And in this assertion we have Physicalism's denial of metaphysical dualism. It is the Logical Positivist's way of saying that psychology must be operational and behavioristic.

As Stevens says, the Positivist merely *asserts* the above—invoking his

mental operations in support of a behavioristic psychology; however, this proposition—if accepted uncritically—at once sets an artificial limit for psychology. It remains for the psychologist to examine such assumptions in good faith—to use mental operations according to a broad perspective on his own particular field—and decide whether or no he is being led toward absurdity. If James's "singular relationship" can be covered with opprobrium by translating *psychophysical* into "metaphysical" the terms of dualism may be converted into something even less intelligible—a semantic limbo which only obscures what the physicalist would deny.

### *Psychological Unification and "Unity of Science"*

A dualistic psychology whose subject-matter, methodology and language must necessarily set it apart from the physical sciences obviously would not be eligible to participate in the long-standing "unity of science" schema proposed by the Logical Positivists and supported by psychology's physicalists. However, it should not be unreasonable to suppose that psychology's first task is to develop a unified science of psychology, rather than to address itself to some larger task of unification. Indeed, it would be inimical to the spirit of science to adopt the language and methodology of physical science before the field of psychology is fully defined.

Conceivably, psychology might be "unified" by attenuation—by systematically ignoring all data which do not happen to fit into a prescribed metascientific framework. Such programs have in fact been advocated, and have met with resistance from those whose perspective on the organismic sphere differs from that of the prescriber. The multiple perspective available to the psychologist enables him to view organisms in terms of phylogenesis, ontogenesis, and orthogenesis, in terms of biology and physiology, behavior and experience, individual and group. He can study the physical universe so as to ascertain the nature of the organismic task of affiliation, and can concern himself with inter-organism relationships; he can consider unobservable neural functions, and such psychic matters as "knowing," "feeling," "imagining," etc.

If no one psychologist studies all of the above aspects of the organismic sphere, there is no reason why they should not comprise a coherent unity in the form of propositional statements which are also consistent with the extrapsychological body of knowledge. Once this level of unification is achieved, the laboratory can be profitably employed in the examination of hypotheses which presently lie latent in the chaos represented by schools, systems and individual idiosyncrasies—all of which serve in an isolative and disjunctive capacity. The material for this task everywhere surrounds the psychologist, being much nearer to hand than the material necessary for a possible unification of *all the* sciences.

## THE IMPLICATIONS OF THE COMMUNICATION PROCESS

That the observing psychologist has at least some experiences similar to those of his subject-matter is hardly to be denied; he would be absolutely incapacitated in any attempt to communicate with his human subjects were this not so. Obviously, this is not true of the subject-matter of physical science in any respect. The fact that at least some of his subjects are known to have "thoughts" and other mental contents which are communicable to him, has always jeopardized the physicalists' position—yet he cannot deny the value of such communication. However, he can circumvent the implications of the communication process by assigning himself an observer-status superior to that of the observed subject—from which altitude his subject's verbalizations may be regarded as "just one more form of behavior" (Spence, 1948, p. 70). Presumably, communication may then be treated as a physical act, or perhaps as an adjunctive tool of the individual—somewhat like the jointed stick in the chimpanzee's grasp. Rather, it may be that the behaviorist thus uses communication in a quasi-physicalistic stick-like manner, so to extract those remote fruits of the subject's psychological center while pretending to concentrate on his observable periphery.

Johnson (1955, p. 11) concludes "that the influence of behaviorism on the psychology of thought has been a negative one." This negative influence stems from naive behaviorism's efforts to circumvent—rather than take into full account—the covert process of *thinking*. But Johnson also notes that "Modern behaviorists, more sophisticated than Watson in some respects, have avoided thinking, preferring first to deal with the simple behavior for which their methods and principles are most suitable" (p. 11-12). Assuming that Johnson intends but a single *entendre*, it can be safely stated that an adequate account of the thought process would have to be interminably postponed if it had to be given in terms of the methods and principles of behaviorism.

Skinner (1957), after an exhaustive analysis of "verbal behavior," gives some consideration to thought processes—not so much because he regards them as an appropriate part of his field of study as because there would be a systematic hiatus if they were not considered at all. For him, "The simplest and most satisfactory view is that thought is simply *behavior*—verbal or nonverbal, covert or overt" (1957, p. 499). It may be informative to regard the approach to communication as "behavior" as it is made by Skinner—who is unique even among behaviorists.

At the outset, he remarks that he is not going to consider verbal behavior from the standpoint of "expression," or "communication"—nor is he going to regard it in terms of the traditional concept of *meaning*. The format of his text entitled *Verbal Behavior* is stated thus:

No effort has been made to survey the relevant "literature". The



emphasis is upon an orderly arrangement of well-known facts in accordance with a formulation of behavior derived from an experimental analysis of a more rigorous sort."—[*made on rodents and pigeons?*]—"The present extension to verbal behavior is thus an exercise in interpretation rather than a quantitative extrapolation of rigorous experimental results (Skinner, 1957, p.11).

Since Skinner is an intelligent and logically-minded human, he produces a schema for verbal behavior which is *internally* consistent; yet he makes no effort to test his schema against the larger context of knowledge about *thought* and *communication*. It can be noted that such internally consistent schematizations thrive on systematic ignorance—a fault not exclusively restricted to behaviorism. Skinner's analysis of what must be regarded as the communication process is made in terms which do not go beyond his principles of operant conditioning. These may well apply to rats and pigeons, but it is obvious that something unique happened to the brain of the primate who was to become *man*. This "something unique" goes far beyond the organism's ability to make mouth-noises, flap lip, and flutter tongue—all of which other mammals can do; it also goes far beyond the mere capacity to associate *sounds* with objects and events or other organisms—which mammals and birds can do as well as man.

#### *What Communication is Not*

The "*language*" of infrahumans is inevitably subjective (Cassirer, 1944) and never *intersubjective*; the experiences of each individual continue privy to itself, and the noises made by the species-membership are elaborated into *signals* utilizable by the subject. The relationship is therefore not between psychology centers, but between a psychological center and the peripheral manifestations—the physical sounds themselves—of a species-fellow. This process is also utilizable *between* species; thus, the deer seems to apprehend the implications of the lion's roar, even though it is obvious that they do not "speak the same language". Similarly, the process which is popularly regarded as "empathic communication"—in which sense it is altogether mysterious—is not, properly speaking, communication. Here again, the respondent organism utilizes the peripheral manifestations of another in signal fashion, and no relationship is established between psychological centers, (cf. Gladin, 1960 b). Finally, the human can use words purely as signals—as when he shouts "*Go!*" to a line-up of foot racers; the effect is not different when he fires a revolver to start a race, or when a traffic signal flashes to *green* in the absence of all humans save the responding automobilist.

One of the fruits of psychology's physicalistic orientation is the widespread belief that *syntactical* verbalization is reducible to terms of physical energy, as if the physical signal-series somehow contained the significance of a communication between psychological centers. For example, in defining the "stimulus," Hilgard (1957, p.28) says:

When . . . a psychologist asks a question and a subject answers, the psychologist's question may be called the stimulus, and the subject's answer his response. In such a case the psychologist does not attempt to describe the precise energy pattern transmitted by his question, *though in principle he might do so*" (emphasis added).

Cassirer (1944, p. 32) observes: "Symbols—in the proper sense of this term—cannot be reduced to mere signals . . . Signals, even when understood and used as such, have nevertheless a sort of physical or substantial being; symbols have only a functional value." When this *functional value* is unwittingly incorporated with the physical signal-series which serves as one of a large number of physical media for transmission—the vocalization—a subtle confusion results. It can be noted that the Braille reader may communicate using dots embossed on heavy paper and thus, without employing physical energy at all, learn the thoughts of minds far and near and of minds whose mortal remains have long since been integrated with the common earth.

#### *Sensory Reception and "Inner Construction"*

The signal-series which is the tangible and observable representative of the communication process is often taken for the *symbol-series* which can only have existence within humans. This occurs because every time a human examines a statement, whether it is spoken or written, his very act of examination adds the crucial element to what is in itself a series of distinctive marks on paper or a serial pattern of sound energy *and nothing more*. James quotes the philosopher, B. P. Bowne, as follows:

No thoughts leave the mind of one and cross into the mind of the other. When we speak of an exchange of thought, even the crudest mind knows that this is a mere figure of speech . . . To perceive another's thought, we must construct his thought within ourselves; . . . this thought is our own and is strictly original with us. At the same time we owe it to the other; if it had not originated with him, it would probably not have originated with us (James, 1890, vol. I, p. 219).

The bare physical signal-series is furnished with its symbolic embodiment when it enters into association with the experiential repertory of the receiver—including first of all his own experience of the relational functions inherent in the syntactical structure of his language; these relational functions determine the serial order of signal production—being the grammar laws which facilitate effective communication. All that is required of the senses is a means of access so as to enable communication to take place, and all that is required of physical energy is a distinctiveness of patterning so as to reduce the possibility of errors of construction by the signal receiver. If physical energy could indeed convey functional values—*significance*—it would be necessary to search for mysterious forces in the physical universe. Yet the notion that the signal-series serves in the capacity of a *stimulus* which is fed into the organism and eventually leads to verbal or non-

verbal response is widespread. It is the *symbol-series* elaborated within the receiver that results in an ideational construction—which then serves in a stimulative capacity; if the construction is accurate, the response will be appropriate; if it is inaccurate, the response will be inappropriate—signifying communication-failure.

*The Human's Departure from "Immediate Experience"*

The development of intentional, to-and-from communication between individual experiencers has produced an organism whose behavioral potential cannot be accounted for in terms of *conditioning*, no matter how ingenious the expositor may be. In turn, the individual experiencer makes use of syntactical language abstractly and relationally—which functions of "verbal behavior" may not be subsumed under laws of conditioning because they are largely mental operations lacking absolute referents in the physical world. Indeed, the communication process enables the individual to withdraw from the world given in sense impressions in order to gain a perspective on the actual world and on himself as well; Piaget (1929, p. 238f.) labels this *dissociation*, which is attributable to "a radical change in the habits of mind."

Cassirer (1944, p. 38ff.) compares the rudimentary and imperfect nature of the infrahuman's abstractive capacity with the *relational and symbol-dependent* reasoning capacity of the human—whose unique achievements are predicted thereupon. We find in man, he says, "a special type of relational thought which has no parallel in the animal world. In man an ability to isolate relations—to consider them in their abstract meaning—has developed. In order to grasp this meaning man is no longer dependent upon concrete sense data, upon visual, auditory, tactile, kinesthetic data." In addition, he has been enabled to isolate relationships *within* symbolic forms themselves—the logic and abstract mathematics which have become essential to scientific discovery and verification (cf. Cassirer, 1944, p. 59ff., 1950, p. 68ff.). Such relationships have no concrete referents in the physical world although the scientist can apply their consequences to physical events.

In his analysis of child development, Piaget (1951, p. 161) says that

... in so far as egocentricity is reduced by the co-ordination which individual point of view with other possible ones, the co-ordination which explains this reduction explains also the formation of logical instruments of conservation (ideas of "groups," systems of relations, etc.) and the formation of invariables in the world of reality (ideas of the permanence of the object, of quantities, weights, etc.).

Acting as commentator, Rapaport notes:

Piaget asserts here . . . the central role of socialization in the development of intelligence. Though this general idea has often been expressed, its concrete statement that communication is both the cardinal means and the index of socialization of thought-organization, appears to be original and important. The significance of communication in particular,

and interpersonal relationships in general, for the development of human thinking, has been little appreciated and less explored (1951, p. 161).

### *Experiential Knowledge vs. Conceptual Knowledge*

James (1890, v. I, p. 221f.) observes that there are two kinds of knowledge: "*knowledge of acquaintance*" and "*knowledge-about*"—which are distinguished in most languages (as in German: *kennen* and *wissen*). That the infrahuman is capable of knowledge of the first sort can be inferred when it repeatedly demonstrates an acquaintance with routes, objects, etc. However, it cannot be shown that the infrahuman is in possession of knowledge-about anything in the universe. James describes knowledge of the second sort as *objective*, or conceptual knowledge, in contrast to what may be interpreted as practical and (ordinarily) *subjective* experiential knowledge.

When communication-dependent conceptual knowledge is treated in the same manner as experiential knowledge—as an attainment of the individual—this oversimplification makes a physicalistic metascience seem beguilingly plausible, and at the same time reduces to illusory status such concepts as "ego," "consciousness," and even "cognition" concepts whose *logical* validity must be sought within the full implications of the communication process. The consequences of fusing the two kinds of knowledge into a single concept—as *experiential* "knowledge"—are not at all obvious, as is attested by the two-thousand year struggle of ontologists who sought to account for the human's knowledge-about an indubitably real universe while persisting in the belief that the human's knowledge perspective is organized out of the experience of the sense-limited individual. On this basis, they were inevitably led to a solipsistic conclusion—the individual could have no knowledge-about an external universe whatsoever.

Of the objectification resulting out of the communication process, de Laguna (1927, p. 260) says: "... *objects* emerge which are cognized rather than felt." The co-ordination of the individual's perspective with the perspectives of others which yields *knowledge-about*—conceptual knowledge of the object-event universe—cannot be reduced to terms of individual learning. Although the acquisition of conceptual knowledge becomes an individual function, no single individual could acquire knowledge-about anything without a prior co-ordination of his perspective with those of others.

*Knowledge of acquaintance*, which by itself can only provide the individual with simple awareness, leads to quite another state of affairs when it is co-ordinated with the *knowledge of acquaintance of others* by means of syntactical abstracts of individual experience—communications. The experiencer arrives upon a new level of awareness, which is expressed in the archaic definition of *consciousness*: "Knowing something together with another." This level of awareness furnishes the individual with an externalized perspective founded in knowledge-

about an object-event universe which enables him to plan and act in a manner not even remotely possible to the infrahuman; and here the logical ground is provided for a scientifically legitimate concept of *ego*—a term often derogated, often used apologetically. Yet it is only when he gains this ego-perspective on the world and on himself as one object among many that the human fulfills the denotational specifications which differentiate him from the general run of infrahumans

### *Communication and the Psychological Metascience*

In consideration of the foregoing material, it is apparent that the communication process cannot be accounted for in terms of conditioning; neither can communication be simply designated as "verbal behavior" and then summarily dismissed as "just one more form of behavior". Also, since the organization of human behavior is so pervasively symbol-dependent, it appears equally indefensible to extrapolate the empirical results of research on infrahumans to the unique human domain on the assumption that "behavior is behavior" wherever it is to be found.

The dynamic aspects of the human condition whose emergence and maintenance intimately depend on the communication process tend to be ignored and even denied when the human is regarded in terms of the typical infrahuman which serves the laboratory. Apparently, however, the current reverence for the laboratory product lends unwarranted dignity to statements whose empirical supports are much less firm than they seem to be. A modern brand of psychology which takes pride in its rigor continues to indulge in a curious form of anthropomorphizing; certain rodent behaviors are designated as "conflict" and "displacement" (cf. Hilgard, 1957) and certain objects or events are labeled as "rewards" and "punishments". Unlike the human who can recognize a relationship with a "rewarder" or "punisher" (and who is often rewarded or punished whimsically by another human who may not even understand his own motivation for so doing) the rodent cannot really appreciate the external agency which benefits or harms him. The application of these terms might be excused as merely an adoption of a convenient nomenclature, but the theorizer reveals otherwise when he extrapolates the results of research on naive animals of limited capacity to the human domain—going so far as to extend his observations to the applied science of psychotherapy (cf. Hilgard, 1956, p. 108, p. 112f.)

Supposedly, the experimenter uses naive animals of limited capacity to investigate basic behavioral functions in their pristine state, yet he is ready to extrapolate his "discoveries" to the world of sophisticated humans, much of whose ordinary behavior is founded in complex familial and extra-familial relationships. That a rat who probably would not recognize his mother if he met her in the "copulatory

reward" compartment of a T-maze, whose father has never been designated to him, and who cannot differentiate among his sibs, half-sibs, cousins, etc., is an appropriate subject to support such gross extrapolations remains highly questionable.

The conventions which psychologists choose to respect may not always be contributive to the best interests of psychology-as-science; empirically derived "evidence" invariably seems more worthy of entertainment than do merely reasonable statements, even though the logical foundations of psychology must rest on propositions which cannot themselves be submitted to empirical test, but which offer a powerful organizing instrument to the psychologist nevertheless. Th articulate spokesmen for a behavioristic psychology recognize the need for some such propositions, and consequently subscribe to the meta-science which gives a physicalistic orientation to their version of the field. Many psychologists are little interested in comprehensive schema, being content to confine their investigations to a limited area; others, while expressing interest, utter the lament: "We don't know enough yet—we must have more research"—seemingly ready to wait for the day when psychology falls into its true path under the sheer weight of its accumulated papers. But psychology does not actually suffer from a dearth of knowledge; rather, it suffers from an insufficiency of application of the available knowledge, which tends to be mislaid as fashions change, ignored when it happens not to harmonize with particularistic emphases, or given a passing lip-service from time to time.

Reference to the full context of available knowledge in the area of communication alone should render suspect the statement that "... the subject-matter of psychology is exactly the same in kind as all other sciences" (Spence, 1948, p. 68; also cf. Carnap, 1935; Pratt, 1939; Davis, 1953). Too, assertions that scant logically significant knowledge about communication, such as the proposition *There is no difference between knowledge and immediate experience*—which Stevens (1939, p. 238) describes as a *psychological* contribution to the philosophy of science—would be dismissed without credit. The common assertion that all sciences are founded in, and operate within, *immediate experience* (cf. Spence, 1948, p. 68)—which is one of the key propositions of "unity of science" advocates—would be adjudged defective from the available context of knowledge also. No other human is so completely dissociated from "immediate experience" as the scientist-at-work; the conventions which enable him to operate ethically and abstractively, his instrumentations, his theories, his goals—all are founded in a community experience which is supra-human in its specialization. Yet, even if the psychologist qualifies as a scientist in these respects, *his science* remains the one responsible for the explanation of how organisms have been enabled to transcend immediate experience and come to grips with the many aspects of



the physical universe which are beyond the reach of ordinary varieties of organismic sensitivity.

### THEORETIC KNOWLEDGE AND "HARD FACTS"

Of the mental operations employed by scientists, Nagel (1944, p. 223) says:

Nowhere is the systematic undervaluation of the constructive function of thought in inquiry more glaring than in the widespread neglect of the role played by symbolic manipulations in scientific procedure. The more comprehensive and integrated a theoretical system is, the more obvious does the need for such manipulation appear. For especially in the theories of modern science symbols usually occur which refer to nothing that can be directly experienced; and the significance for matters of direct experience of the conceptual constructions which enter into these theories cannot be made explicit except with the help of extensive symbolic transformations.

The physicist has never seen an atom; his "picture" of atomic structure is a mathematical representation—a coherent relational model whose elaboration must take into account strictly *secondary* evidence (spectra, effects, "tracks") of otherwise unobservable atomic activity. Initially, the significance of much of this type of evidence was far from obvious; if it seems obvious now, this is because the correct interpretations were placed on apparently isolated physical events. The validity of the relational model of the unobservable atom is maintained by the empirical demonstration that indirectly verifiable consequences which should issue as a function of specifically stated relationships do in fact so issue. The primary restriction on the physicist's employment of mental operations is the demand that the sum total of his propositions be consistent among themselves, that they take full account of the relevant empirical data, and that they remain open to question on the basis of future information.

Among behaviorists, the extremist position holds that only observable behavior—including verbal reports but not their experiential content—constitutes the fitting subject-matter of a scientific psychology. This viewpoint accords well with the narrow empiricist philosophy of science which holds that there must be a sensory warrant for every proposition of a science; nevertheless, it may seem unreasonable to demand more of the psychologist than of the physicist—and particularly since the psychologist's subject-matter does have a subjective core which, although it is not directly observable, has long been regarded as uniquely significant.

"Psychology is the only discipline in which the question of why things *appear* as they do is asked," Allport (1955, p. 55) remarks, pointing out that the task of the perception psychologist is to observe the process of observing nature. Recognizing the necessity of employing the "rule of objectivity" wherever it can be employed, he suggests, "... perhaps we can make an exception in the study of perception be-



cause we must." Again, it may seem unreasonable to demand that a science defer to convention rather than to the particular nature of its subject-matter. In deference to the nature of *its* subject-matter, nuclear physics has, over several generations, devised a remarkable complex of instrumentation in order to yield up the invisible to observation; it did not begin its quest by swearing allegiance to the tangible.

Cassirir ( 1944, p. 58f. ), the philosopher of symbolic processes, says:

Empiricists and positivists have always maintained that the highest task of human knowledge is to give us the facts and nothing but the facts. A theory not based on facts would indeed be a castle in the air. But this is no answer to the problem of a true scientific method; it is, on the contrary, the problem itself. For what is the meaning of a "scientific fact"? Obviously, no such fact is given in any haphazard observation or in a mere accumulation of sense data. The facts of science always imply a theoretical, which means a symbolic, element. Many, if not, most, of those scientific facts which have changed the whole course of the history of science have been hypothetical facts before they became observable facts. When Galileo founded his new science of dynamics he had to begin with the conception of an entirely isolated body which moves without the influence of any external force. Such a body had never been observed and could never be observed. It was not an actual but a possible body—and in a sense was not even possible, for the condition upon which Galileo based his conclusion, the absence of all external forces, is never realized in nature. It has been rightly emphasized that all the conceptions which led to the discovery of the principle of inertia are by no means evident or natural; that to the Greeks, as well as to the men of the Middle Ages, these conceptions would have appeared as evidently false, and even absurd. Nevertheless, without the aid of these quite unreal conceptions Galileo could not have proposed his theory of motion; nor could he have developed "a new science dealing with a very ancient subject." And the same holds true for almost all the other great scientific theories. Upon first appearance they were invariably great paradoxes that it took unusual intellectual courage to defend.

Both Nagel and Cassirir point out the necessity for going beyond the "hard facts" in order to obtain scientifically factual knowledge. The prodigious accomplishment of Einstein—often cited as the logical basis for a strictly physical operationism—is founded in systematic imagination: the conception of a kind of space beyond possible sensory experience. Is it probable that a science as comprehensive as psychology has no legitimate interest in the study of—as well as the employment of—these essentially mental operations? Or is it only by reason of default to specious authority that "... practically all psychologists agree that only physical phenomena are the material that psychology, like any other natural science, is concerned with" (Bergmann and Spence, 1951, p. 57—footnote.)

#### *Psychology—the Definition of a Science*

A contemporary introductory textbook states: "Psychology may be defined as *the science that studies the behavior of man and other animals* . . . By *behavior* we mean those activities of an organism that can be observed by another person or by an experimenter's instruments" (Hil-

gard, 1957, p. 2). Although this definition is intended for the naive student, it is one widely accepted *in principle* by sophisticated psychologists. Strangely, it seems to be self-consciously addressed to the status of psychology as a science rather than to the comprehensive nature of the psychological subject-matter.

If the definition refers to the physical acts of the psychologist, it may be altogether true that he *studies* only the tangible; however, if what is to be meant by "psychology" also covers his mental operations, his field is considerably broadened. The often-neglected psychological manifold includes much theoretic knowledge—knowledge which is in no wise available to simple sense, yet indispensable to a complete understanding of what the organismic complex is and how it accomplishes what it does.

Regarding the definition as it stands, it may be readily determined that a purported "science of behavior" does not even have uniform agreement as to what the term *behavior* means. Implicit acceptance of the definition leads to the unwarranted assumption that anything organismic observed by, or studied by, a psychologist constitutes "behavior," and that any observable activity of an organism automatically classifies as "behavior". Since many of the more significant facets of organismic functioning are concealed beyond the periphery accessible to sense, the specification of *observable behavior* carries the implication of covert activities designatable as *unobservable* behavior. Thus, glandular and alimentary functions, receptor adjustments, neurological activities, muscular movements, imagination, thought, affect, etc., are broadly classifiable under the single rubric of "behavior." Although it might be presumed that at least some of these functions are not in any wise *behavior*, their definition rests with the student.

Those "students of behavior" whose traditional orientation has been toward the observable periphery of organisms consciously extend behavior into the organismic interior in order to reduce the scientifically reprehensible psychological center to the terms of its observable periphery. Thus, *thinking* becomes "implicit verbalization," or "thinking behavior," and *imagery* and *perception* become "central responses," under the explicit assumption that the unobservable is answerable to the "laws" of the immediately tangible. In Miller's words (1959, vol.2, p. 243): "... we clearly assume that these central processes follow the same laws as do peripheral stimuli and responses."

The kind of definition which fully and accurately describes psychology surely need wait on no ultimate determination based on knowledge yet to come; as surely, it need not be limited by obfuscating traditions which purport to represent the scientific. At any rate, it can be safely stated that psychology is not and never has been strictly a science of *behavior*—a term which implies the given, the tangible, the immediately observable. The pertinent question has always been: what

is *behind* behavior? The psychologist's laboratory has no other reason for being than the pursuit of answers to this question, and he neither stages nor observes behavior samples for their own sake. If he continues to subscribe to a semantic convention which defines his supposed field of study, he does no more than to acknowledge the immature status of his science in the very act of asserting its scientific dignity.

### THE ORGANISMIC TASK OF AFFILIATION

The *relation of knowing*, says James (1890, v. 1, p. 216) "is the most mysterious thing in the world." The problem of knowledge has been a primary source of concern for philosophers for many centuries. Since in this area philosophers have been acting—often naively—as psychologists, the problem is one for psychology to resolve. Its essence is represented by the paradox that organisms act as if they were possessed of at least some knowledge of a surround external to themselves and yet are effectively sealed off from *direct* apprehension of the physical domain. As Bowne remarks, "... when we conceive the mind as coming into contact with the outer world only in the dark chamber of the skull, and then not in contact with the objects perceived, but only with a series of nerve changes of which, moreover, it knows nothing, it is plain that the object is a long way off" (James, 1890, v. 1, p. 220).

Save for a few exceptions (e.g., Skinner's *operant conditioning*, in which response is primary and Tolman's *sign learning*, in which meaning is primary) behaviorists have elected to make the affiliation of organism and surround dependent on the instigatory effects of physical "stimuli": Although Dewey (1896) observed some generations ago that the organism *constitutes* its stimuli, and psychology's holists have long protested against the stimulus concept, this notion has become firmly embedded in the psychological literature. Rare is the student who has not been solemnly indoctrinated with the concept of physical energy's instigatory role as regards response *qua* behavior before he leaves the first chapter of his introductory text; perhaps as rare is the sophisticated psychologist who has managed to unlearn this mischief in the face of its pervasive usage elsewhere.

The gross misconstruction of the physical facts abstracted from the knowledge of the physicist and physiologist seems responsible for the formal and apparently scientific animation of physical energy as "stimuli" and the obvious tangibility of their data appears to be sufficient reason for cherishing the concept in the face of contrary knowledge. Himself having knowledge-about objects, the psychologist discovers the physicist's knowledge-about energies which emanate, or are reflected from, such objects. Since such matters can be regarded in the abstract by science—i.e., independently of the organismic sphere—this independability of description provides the psychologist with the logical ground for conceptualizing the "independent stimulus variable". Turning next to the physiologist, he discovers the effects of energetic stimulation

on receptor-neural tissue in the form of graphic records of electrochemical nerve impulse activity. Lo! Tangible evidence of causal progression from the physical world into the organism. And sufficient as a basis for an empirical "input-output" psychology founded in the conceptual focus of attention of fallible humans.

The actual behaving organism's receptor periphery continuously undergoes a bombardment of physical energy far in excess of what the stimulus-response theorist chooses to regard as actual or potential "stimuli". That portion which he selects out for special attention is associated with objects and events which have become significant to organisms, hence it *seems* to evoke behavioral responses (cf. Gladin, 1960a). Fixing on the alterations of the laboratory animal's surround which he knows are followed by differential excitation of the organism's receptor systems and then by "behavior changes," he overlooks the fact that alterations are continuously taking place beyond the organism's sensory periphery—alterations which are always followed by differential excitation but *not* always by "behavior changes". But even the differential excitations cannot be said to be determined from the physical sphere whose energies are indiscriminately diffused in all directions: any differential must be determined from a point location in physical space occupied by an organism. Within "the dark chamber of the skull" the complexly structured vertebrate must organize a utilizable reflection of the external surround—in terms of fluctuating foregrounds and backgrounds—by reducing to a semblance of order the diffuse and pervasive bombardment of essentially indifferent physical energy. If the mechanisms for this accomplishment were structured during the phylogenesis of those vertebrates which are presently subject to human observation, their functional utilization during ontogenesis continues to depend on the organisms themselves, never on the indifferent physical universe.

At the human level, it is not unreasonable to suppose that a transcendental kind of knowledge of a universe of objects and events has a great deal to do with the instigation of behavior. The human's access to a near-infinity of conceptualized possibilities must invoke in him a curiosity quite unlike the "exploratory drive" of the naive rodent introduced into its first maze. His knowledge-about himself and other humans, his consciousness that other humans have at least some knowledge-about him, are more probable determinants of his behavior than physical "stimuli". Add to this his knowledge-about predictable and unpredictable orders of events occurring in his systematically—if artificially—organized and conceptualized temporal and spatial extensions, his consciousness of the past and of a future transcending his own mortality, and doubt should be further reduced.

Regarded strictly as a *tool* of the psychologist, the notion of a stimulus may have some specific utility; regarded as a theoretical concept having to do with organism-universe affiliations, the notion is

invalid and its continued usage in this way is a gross violation of the scientific and nothing less than that. If it seems to be supported by a physicalistic metascience, that metascience must also be invalidated for its espousal of pseudo-knowledge as empirical fact.

### SUMMARY AND CONCLUSIONS

The present paper has been concerned with a series of issues having to do with the possible establishment of a metascience addressed to the special problems of a unique science, psychology. Under the assumption that psychology is not a physical science, dualism was advanced as the forte of its uniqueness and supported on the grounds of its inevitability in a science which has to do with an organismic domain whose subjective core has persisted in remaining significant. The communication process was discussed at length in terms of its implications for the sciences in general and psychology in particular. The nature and function of knowledge was considered as it bears on psychology's metascience and necessary definition, and with regard to the human's task of affiliation with his animate and inanimate surround.

Since it is supported by an imposing physicalistic metascience, the behavioristic version of psychology was examined all along the way. On the premise that there is sufficient available knowledge to enable the psychologist to determine whether a particular direction offered him leads to a cul-de-sac or no, evidence—conceptual and otherwise—was presented for critical evaluation, albeit with obvious personal bias.

In this author's opinion, the time seems ripe for an earnest attempt at the unification of psychology into a comprehensive science. This is not a task to be accomplished by a mere compilation of yesterday's papers like so many chapters of holy writ, chaff and wheat unsorted; nor is it a task for a collection of humans with axes to grind, as so often symposia turn out to be. It is a monumental task involving the dispassionate examination of various positions without regard for the persons espousing them and in the full light of available—if not always empirical—knowledge. Also required is the critical examination and sorting of the appalling accumulation of insensate data whose periodical issuance is ever on the increase in an inexorable, chaotic flood. Herein, it has been asserted that psychology has the necessary knowledge to construct a coherent frame of reference into which the wheat of facts and ideas can be directed without the present burden of chaff. By organizing the subject-matter of psychology into a coherent whole—itsself consistent with the extra-psychological body of knowledge—psychologists should become enabled to function in a spirit of harmony appropriate to a full-fledged science. This is the imperative, the inevitable goal of a scientific psychology—one to be accomplished later if by trial and error or sooner if pursued with conscious purpose and in good faith.

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## A CLASSROOM DEMONSTRATION OF "EXTRA SENSORY PERCEPTION"<sup>1, 2</sup>

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The demonstration to be described in the present paper is one that the author has used successfully in his classes for a number of years. It is always presented as a problem-solving situation which the students are urged to come to grips with and to solve with the analytic tools of scientific psychology (such as stimulus-response, setting factors, etc.). As an introduction to the demonstration, students are told that just as engineering or chemistry has questions that must be answered and as algebra or geometry has its problems that demand solution, so what they are about to witness must be studied so that they understand *what* has happened and *how* it happened.

The instructor then takes a deck of ESP cards, preferably of the earlier edition and hands them to a student at random in the first row of the class. He requests the student to shuffle the cards thoroughly and to ask one or more other students to cut the deck. He then asks the student who shuffled the cards to place the deck face down on the table that is usually found at the front of a lecture room. He further instructs this same student to sit close by the cards and to keep his hands off the deck until the experimenter calls each card in turn, at which point the card handler must hold up the card so that the class may clearly note each hit (or miss) of the instructor's call.

After the instructor surrenders the deck of cards to his assistant, at no time must he come into contact with the cards until after the demonstration is over. In fact, to be most effective, from the time he hands the cards over to the assistant, the instructor walks back away from the table about six feet and stays in this position. (See Fig. 1). He then calls the cards properly, practically as fast as his assistant can turn them, to the complete amazement of his audience.

*How the trick is done.* A number of years ago, the writer stumbled across the fact of his "ESP powers" as he looked at a deck of ESP cards, face down, several feet away, on his desk. To his surprise, he found that under the existing conditions of illumination he could make out the symbol on the back of the card. Analysis of the situation showed that the light reflected from the back of the card reflected dif-

<sup>1</sup> Appreciation to Mr. Gerald Brazil for help with his photographic skill is hereby gratefully acknowledged.

<sup>2</sup> The author wishes to express his gratitude to Dr. Margaret Habein, Dean of Liberal Arts, University of Wichita, for her financial support of the present study.

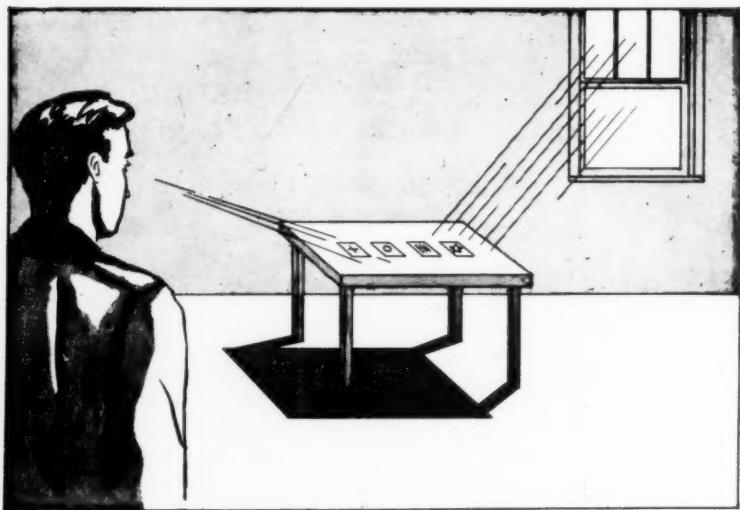


Fig. 1. A representation of the relationships that must obtain among the three variables, (a) illumination source, (b) position of ESP cards viewed face down, and (c) the human observer.

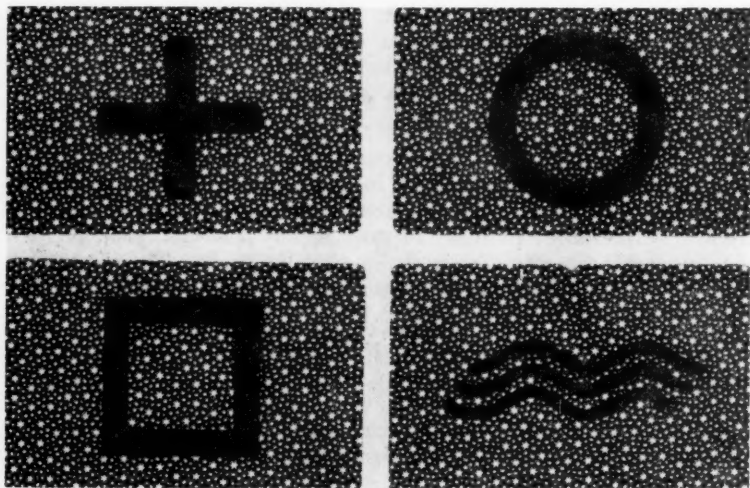


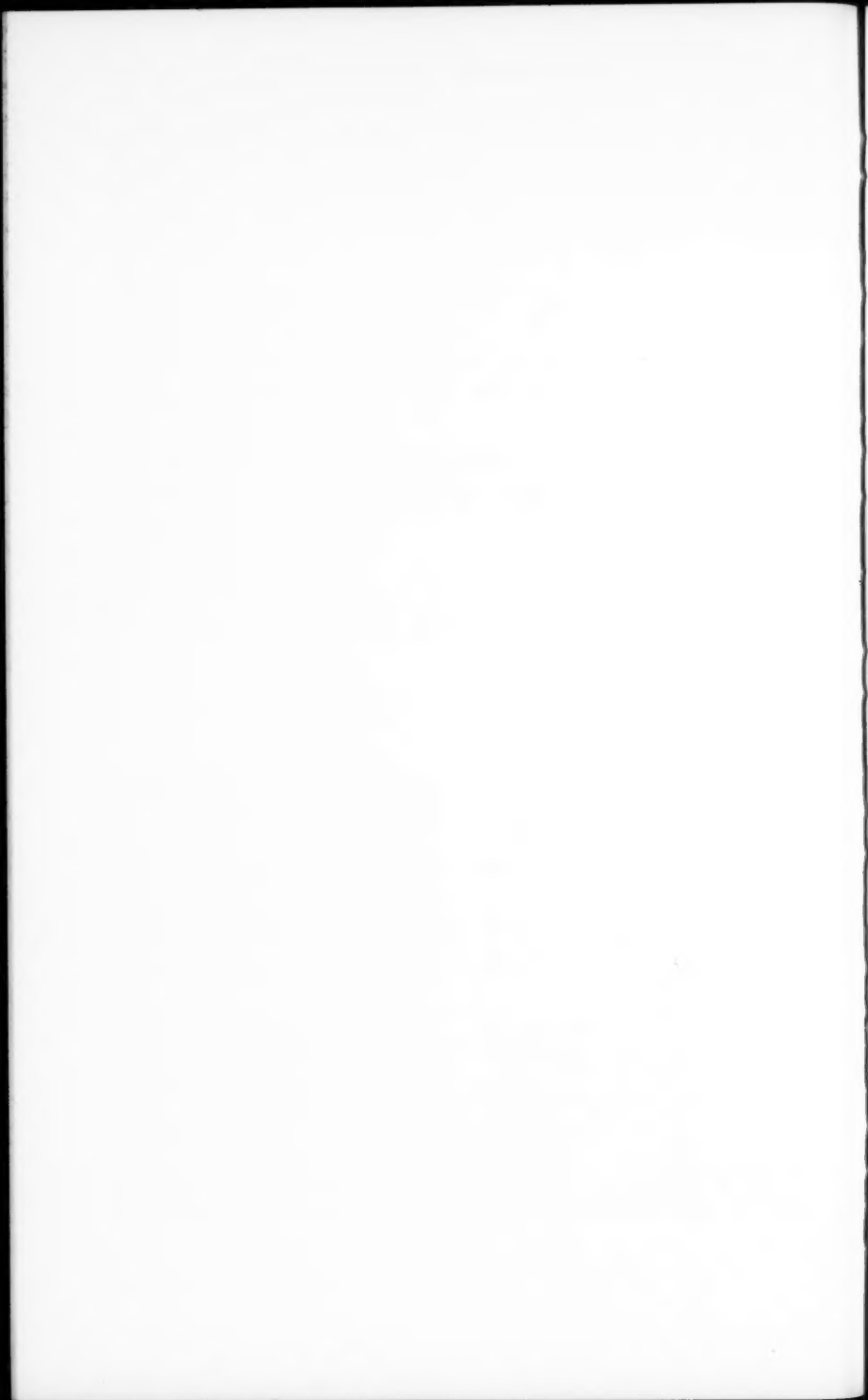
Fig. 2. A photographic artifact which attempts to represent how the symbols of the ESP cards appear on the back of the card as they are perceived when placed face down. However, it was impossible to capture photographically the subtle stimulus configuration on the back of the card nevertheless perceivable by the human subject.

ferentially from the area in which the symbol had been imprinted on the under side (i.e., the face of the card). (See Fig. 2). Continuing with the rest of the deck, he had equal success, predicting the symbol on each card *before* turning it face up. Fired with such quick success, he continued practice under varied conditions permitting him to build up quick and subtle (non-extra) sensory perceptions. The only precaution necessary prior to demonstration (i.e., before the class or group assembles) is to make a quick check of the illumination for best placement of the cards and the demonstrator. With these conditions fulfilled, no amount of card shuffling or cutting the deck can confound the experimenter's "powers." Perfect runs are the rule and even a single miss out of 25 calls is aggravating! The problem has been consistently difficult for the class to solve.

It should be stressed that the earlier printing of the ESP cards is preferable to the more recent issue. However, the writer is making progress in developing reaction sensitization to cards of the later printing.

#### SUMMARY

A classroom demonstration of what appears to be extra sensory perception achieved through inapparent, plain "sensory perception" is described.



## THE MEASUREMENT OF HYPNOTIC EFFECTS BY OPERANT-REINFORCEMENT TECHNIQUES<sup>1</sup>

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Operant-reinforcement procedures were used to produce a base-line performance for evaluating the effects of hypnotic suggestion on a student nurse. The performance was matching-to-sample, a procedure which has been studied extensively in infrahuman subjects.<sup>2</sup> The apparatus consisted of three stimulus-display windows, each 1½ by 2 by 1½ inches, in which eight geometric patterns and colors could appear. The subject pressed telephone-type keys, which were mounted under each window. At the start of each trial, a stimulus appeared in the center window. When the subject pressed the center key, the center window became blank and stimuli appeared in the two side windows. One stimulus matched the sample stimulus which had just previously appeared in the center, and the other did not. Furthermore, both the center-window stimuli and the arrangement on the side keys varied from trial to trial in a random manner. The subject was reinforced for pressing the key under the stimulus that did *not* correspond with the sample that had appeared in the center window. All correct responses were reinforced by the brief flash of a green light, but only intermittently by the advance of a counter just above the center window. The counter advanced on a variable-interval schedule with a mean of 3 minutes and a range of 2 seconds to 6 minutes. The conditioned reinforcer (the green light) reinforced each matching sequence and maintained the matching-to-sample sequence as a larger unit of operant behavior. The intermittent-reinforcement schedule produced a base line with a large amount of behavior maintained by only occasional reinforcement. Pressing the key under the matching stimulus (incorrect responses) produced a 6-second black out of the entire apparatus. After either a reinforced response or a black out, a stimulus reappeared in the center window and the procedure was repeated. The subject was paid at the end of the session at a rate of \$0.20 for each tally on the counter. At the maximum rate of delivery of reinforcement, subjects could earn \$4.00 per hour.

An experimental session lasted 2 hours, and a stable base-line performance was established before the hypnotic procedures were intro-

<sup>1</sup>This experiment was carried out under Grant G-7617 from the National Science Foundation.

<sup>2</sup>Skinner, B. F. Are theories of learning necessary? *Psychol Rev.*, 1950, 57, 193-216.  
Ferster, C. B. Intermittent reinforcement of matching to sample in the pigeon. *J. exp. Anal. Behav.*, 1960, 3, 259-272.

duced. The variable-interval reinforcement of the matching-to-sample procedure produced a constant rate of responding, as would be predicted from this same schedule of reinforcement with infrahuman subjects on a similar matching procedure. The stable state was achieved very rapidly, after only 5-hour exposure to the schedules of reinforcement before the performances were sufficiently stable and predictable so that the hypnotic procedures could be introduced. The hypnotic procedures were carried out by strengthening already-established hypnotic patterns. In earlier experiments, various behaviors and states, including anxiousness, had been induced by hypnotic suggestion.<sup>3</sup> The subject received an additional hour of training, just before this experiment, during which she was trained to respond rapidly to various hypnotic suggestions. The actual stimulus used to evoke anxiety hypnotically was a shortened version of a suggestion which had been used in the earlier experiments. Figure 1 describes the sequence of experimental procedures during the experiment as well as the behavioral effects of the

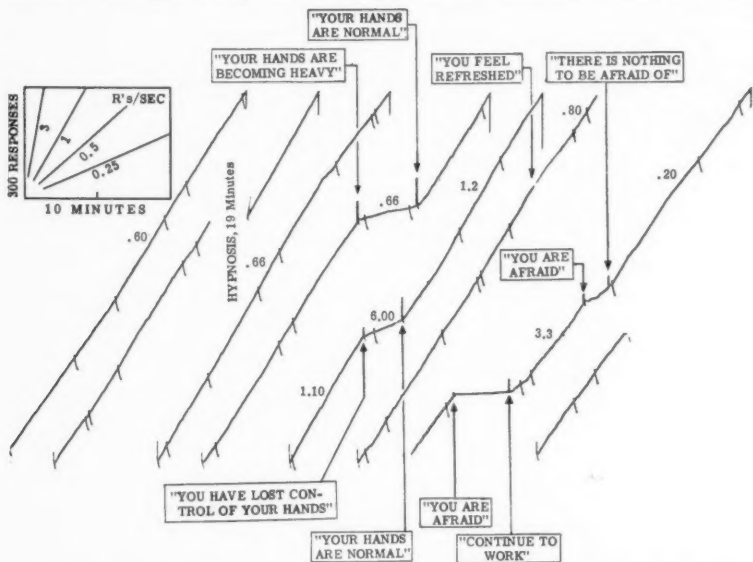


Fig. 1. Cumulative-response record of a subject's matching to sample under various hypnotic procedures. The completion of each matching-to-sample sequence advanced the response scale of the recorder one step. The oblique marks indicate the advance of a counter whose total determines the subject's rate of pay. The numbers above the curve indicate the number of errors per minute. The text in the boxes in the body of the figure give the entire protocol of the hypnotic suggestions carried out while the subject was matching to sample.

hypnotic procedures on the performance. The first two segments of this figure show the matching-to-sample performance during the first 30 minutes of the experiment, before the subject was hypnotized. At this time, the subject's rate was a little less than 1.0 response per second,

<sup>3</sup>Levitt, E. E., den Breeijen, A., and Persky, H. The induction of clinical anxiety by means of a standardized hypnotic technique. *Amer. J. Clin. Hypnosis*. 1960, 2, 206-214.

and only 0.6 error per minute occurred. The subject was then hypnotized in another room in a 15-minute session, and was returned to the experiment immediately following induction of hypnosis. Now, her base-line matching-to-sample performance was examined under hypnosis but without any explicit suggestions for an additional 30 minutes. The second two segments of the record show that the subject's performance was essentially the same as her initial performance without hypnosis, both in over-all rate of responding and number of matching errors.

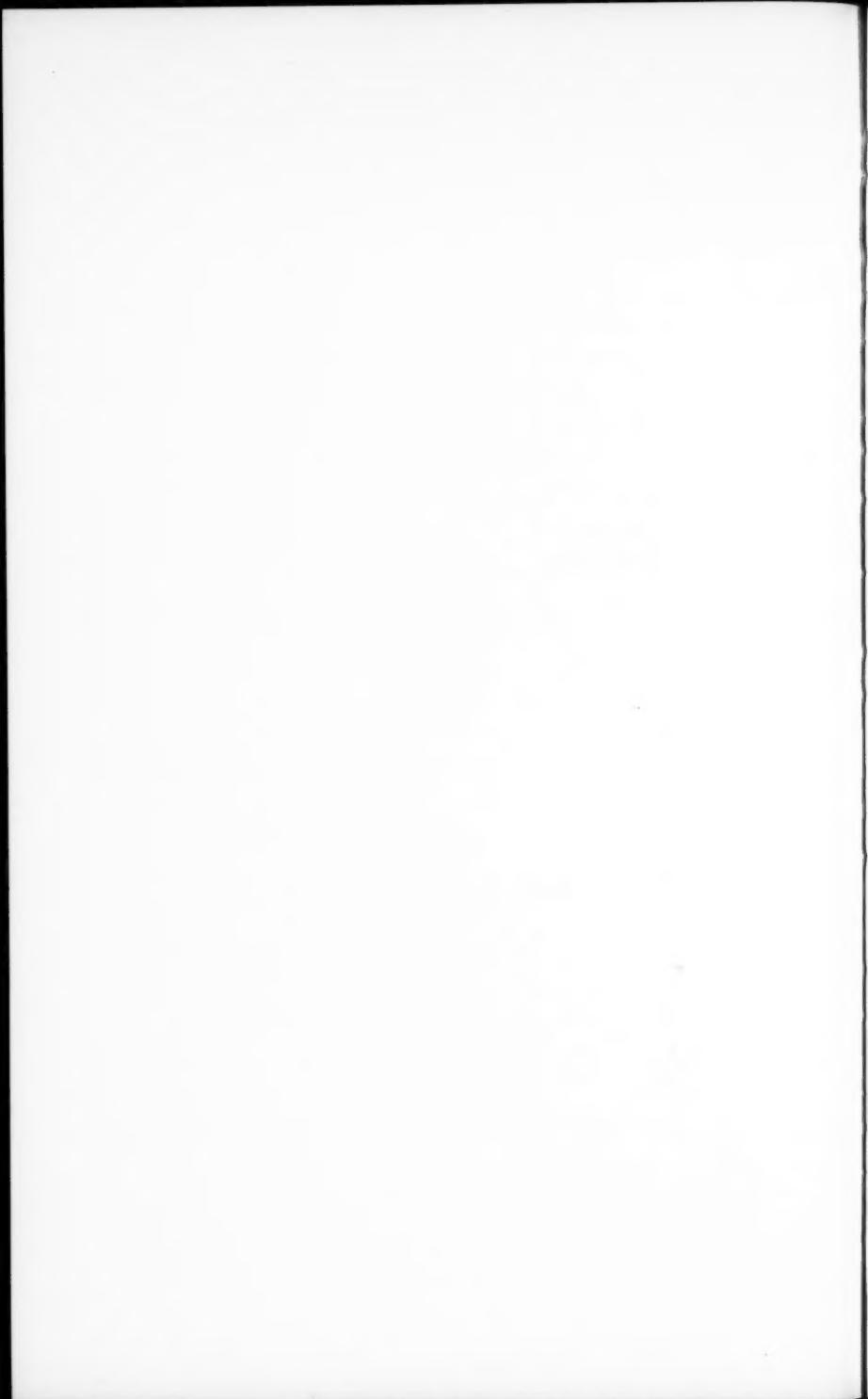
The first suggestion was, "Your hands are becoming heavy"; and the result was an immediate decline in the rate of responding, from approximately one matching sequence per second to approximately 0.1 matching sequence per second. The subject lifted her hands with great effort, sometimes using one hand to lift another. The number of matching errors, however, did not increase over the level recorded, either before the hypnosis or under hypnosis but without any suggestion. After 4 minutes, the subject was told, "Your hands are normal"; and the over-all rate of matching returned to the base-line level. The rate of errors increased, however, to 1.1 errors per minute.

Ten minutes later, a second suggestion was made: "You have lost control of your hands." The over-all rate of matching declined immediately to about 0.25 match per second, a little more than twice the rate recorded after the first suggestion. However, the number of errors increased to 6.0 per minute, almost a tenfold increase over the error rate after the first suggestion.

The base-line procedure was continued for another 30 minutes before the next suggestion, "You are afraid." The subject showed visible signs of emotion; and, except for a sporadic response, she stopped working for almost 4 minutes, when she was instructed: "Continue to work." For the next 6 minutes, the over-all rate of matching was only slightly lower than the base-line levels of responding, but the rate of errors was 3.3 per minute. The suggestion that the subject was afraid was then repeated, and the rate at which the subject worked fell to about 0.5 match per minute, approximately half of the control values. Two minutes later, the subject was instructed: "There is nothing to be afraid of." The rate of responding again returned to control values, and the rate of errors was the lowest recorded during the session.

We tested these procedures on other student-nurse subjects, and the results were all similar to those reported here. The sensitivity of the operant performances to the hypnotic procedures has provided a technique for objectively analyzing some of the properties of hypnotic suggestion. The two aspects of the dependent variable, the frequency of emission of the behavior and the number of errors, varied differentially as a result of the different suggestions; and they provided a base line that was more sensitive to manipulation than the simple response often recorded in operant research. This technique will allow us to investigate such variables as the duration of effectiveness of a hypnotic suggestion or the relative efficiency of various hypnotic procedures.





BEECHER, H. K. *Measurement of subjective responses: quantitative effects of drugs*. New York: Oxford University Press, 1959. Pp. 492.

Question: when is an anesthesiologist a psychologist? Answer: when his name is Henry Beecher, who has devoted a generation to the study of subjective response in man. What was good enough for Wundt and Titchener is equally good for Beecher, despite the number of contemporary psychologists and other behavioral scientists who seem to frown upon verbal report of sensation as a legitimate class of behavior.

This volume presents a general summary and integration of clinical investigations from Beecher's laboratory at Massachusetts General Hospital (Beecher holds the position of Henry Isaiah Dorr Professor of Research in Anaesthesia at Harvard University.) Most of the content has previously been published in article form, so for someone who has kept abreast of Beecher's work the book does not offer much that is new; in fact, at times the writing becomes rather repetitious, with many of the points appearing and reappearing in different chapters. Nevertheless, the uninitiated reader will find a fascinating account of a truly scientific approach to a variety of problems in clinical psychopharmacology.

With extensive documentation (including a reference list of 1063 publications), Beecher guides the reader through a domain which—originally the province of the medical research worker, but more recently utilizing the efforts and skills of the behavioral scientist—literally tangles with basic and applied clinical problems. Although the content centers around the use of drugs and the evaluation of "symptoms" (i.e., subjective responses), one need not be basically interested in psychopharmacology *per se* to appreciate the research approaches and contributions described throughout the book. For example, the psycho-clinician will be particularly attracted by the evidence adduced to support the notion that a behavioral pathological response induced experimentally may be quite different from the same response spontaneously observed clinically (e.g., laboratory-induced pain vs. post-operative pain). Similarly, Beecher argues convincingly the need to differentiate the effects of a treatment upon a *response* to a sensation from the effects upon the sensation itself.

Much is made of the need for placebo controls in evaluating the effects of pharmacological agents, and Beecher deserves unqualified credit for his emphasis on rigor of clinical research design. In his chapter on "Placebos and placebo reactors," however, it is this reviewer's belief that Beecher lowers considerably his standards of what constitutes reliable evidence, and tends to oversimplify the state of our present knowledge concerning the determinants of placebo response: our ability to identify stable, consistent personality types who may be labeled

"placebo reactors"—independent of situational determinants—is still so highly questionable that the concept itself may turn out to be more misleading than useful.

Mention should also be made of a chapter on "Statistical problems and their solution," specially written for the book by F. Mosteller. This chapter cannot be recommended as a primer on experimental design for those embarking upon a research career in clinical psychopharmacology. In the limited space permitted him, Mosteller seems to have chosen but a few particular problems to dwell upon (possibly of current interest to him and to Beecher), while ignoring many others. The level of writing is quite variable, requiring a statistical background in some portions and merely good common sense in others.

Within the chapters of this book, the reader will find accounts of experiments on animals and humans, the latter including normal healthy subjects and patients. As in any other area of behavioral science, an integration of all findings is necessary for an adequate understanding of one's subject matter. Conspicuous throughout, however, is the stringent use of quantitative methods along with rigorous procedural controls as applied to clinical problems. It should encourage the clinician to know (as he already should know) that whether the question involves psychotherapy, pharmacotherapy, or any other form of "treatment," both the basic and applied aspects of the problem can be attached meaningfully in clinical situations. Clinical research need not be sloppy, unreliable, and equivocal; nor need it be overcomplicated. But it must be sound. And Beecher's book offers much of value to the research psychologist who values soundness.

National Institute of Mental Health

SEYMOUR FISHER

UHR, L. and MILLER, J. G. (Eds.) *Drugs and behavior*. New York: Wiley, 1960. Pp. 676.

In *Drugs and Behavior* we have the Whitman's Sampler of psychopharmacology. As such, the volume offers a bit of something for everyone, and thereby fills a gap created by the infant age of a field which has few available standardized reference works. For this reviewer, the book has solved a frequently recurring personal problem: what do you tell the graduate student (be he in psychiatry, medicine, pharmacology, or similar fields) when he asks for a convenient introduction to the nature of psychopharmacology. Previously, I have always sent him scurrying to Coe and Gerard's *Psychopharmacology: problems in evaluation*, with apologies for the lack of additional reference texts (the major psychopharmacological contributions being scattered throughout an astounding variety of scientific journals). Now I can add the Uhr-Miller volume to my recommendation—with the same apologies. Taken together, the two books will give as fair an idea about the kinds of activities (research and clinical) which delineate psychopharmacology as can be obtained without recourse to the journals. It is interesting

to note that the Cole and Gerard volume, based upon a conference held in September, 1956, contains most of the pertinent issues which appear in various chapters of the 1960 Uhr-Miller book. Alas! It appears as though our problems do not get solved very readily.

*Drugs and Behavior* contains contributions by 63 investigators, these chapters varying in length from one and a half to 46 pages. The papers are organized into "(a) background, discussion, and theoretical papers, and (b) reports on specific researches conducted by the author." The major sections include papers on (1) biochemistry, physiology, and pharmacology; (2) methodology; (3) clinical considerations; (4) experiments on animals of potential application to human subjects; (5) objective assessment of normal human behavior; (6) techniques for assessing autonomic, motivational, and pathological states; (7) experimental use of observational techniques; (8) controlled subjective measures; and (9) a summary. Aside from these necessarily loose categories, each paper represents an unintegrated "bit," and the reader can merily whet his appetite by skipping around to chapters whose titles arouse his interest. The emphasis is heavily upon work with human subjects, both normals and patients, although as noted above, one section is devoted to animals.

A volume of this sort cannot really be reviewed. The easy way out would be to reproduce the table of contents and leave it at that. On the other hand, it would be well worth while for the reader to do his own sampling. The bon-bons which I particularly liked—if my biases may be forgiven—were Lorr's fine compilation of available rating scales and behavior inventories, Holliday's study of experimentally controlled stress and drugs effects, and the contrast between Lehmann's and Nash's approaches to the use of controls and objective methods in psychopharmacology. I was disappointed in Uhr's rather uncritical summary dealing with objectively measured behavioral effects of psychoactive drugs.

Dip in and have a bon-bon. Some are quite tasty.

*National Institute of Mental Health*

SEYMOUR FISHER

WHITE, MARY ALICE and HARRIS, MYRON W. *The school psychologist*. New York: Harper Brothers, 1961.

Focusing "on the people, issues, and responsibilities with which the school psychologist must deal," the present work is "a textbook, a handbook, a manual" which offers very practical help to school psychologists. Problems attacked ranged from those concerning the school administration, the educational process and school society, and maladjustment among pupils, to psychodiagnosis, therapy, and the writing of reports and research.

*University of Wichita*

N. H. PRONKO

EYSENCK, H. J. (Ed.) *Handbook of abnormal psychology*. New York: Basic Books, 1961.

As implied by the title, this is both an extensive and intensive survey of the research literature covering the field of abnormal psychology. Except for one contributor now in this country, all are associated with research institutions, clinics, and departments of psychology, throughout the British Commonwealth.

Its coverage runs the gamut from birth (including genetics) to senility, behavior disorders to the psychoses, animal to human behavior, and instincts to learning processes. In general, there tends to be a pessimistic feeling throughout to the effect that neither psychology nor psychiatry, to date, have accomplished much via either their research or therapeutic efforts. The major exception appears to be the factor analytic approaches which, however, like a "voice in the wilderness" go disregarded and unheeded by all those but its adherents.

As a text it lacks a unifying theme other than its title and, perhaps, those chapters where the factor analytic studies are stressed and favored. It is not then, a book readily used for teaching purposes but, on the other hand, would be extremely useful to persons about to undertake almost any research problem in abnormal behavior or graduate students in need of a comprehensive coverage of the research literature.

*Mental Health Institute*  
Mount Pleasant, Iowa

JAMES W. LAYMAN

BARBU, ZEVEDEI. *Problems of historical psychology*. New York: Grove Press, Inc., 1960. Pp. x + 222.

Most psychologists will agree with the author's view that close cooperation between the disciplines of psychology and history can open up "new and fruitful perspectives for the understanding of both fields." Certainly many will accept the view that the human mind is historical in character; that complex human behavior is a function of culture, and that culture has had a long historical evolution. At the same time, however, few psychologists have done more than to pay lip service to this idea. Analyses of character and personality in terms of present day cultures are not uncommon, but descriptions of unique personal patterns as a function of historical development are rare.

Two chapters are devoted to suggestions regarding the impact of history on perception and emotionality. Of particular interest is the analysis of the shift toward a great reliance upon visual perception in the modern world and of changing artistic perception as concomitant with changes in the organization of the inner life.

Possibly the most interesting chapter in the book is one dealing

with the emergence of individuality in the Greek world. This chapter is filled with hypotheses and data, sometimes fascinating and always interesting and suggestive. The final two chapters treat the origins of the modern English character in the sixteenth century. While the assumption that a national character exists will be challenged, the author has presented a clear, strong case for such a point of view.

It goes without saying that the author has no sympathy with doctrines which stress the fixity of the human mind or with reductionist positions which would explain mind in terms of non-mental factors. The fruitfulness of a dynamic, non-reductionist analysis in this instance makes one hope that a comprehensive psychology stressing historical factors, as they operate both in this individual and in the group, is at last emerging.

Denison University

PARKER E. LICHTENSTEIN

PAVLOV, IVAN P. *Experimental psychology and other essays*. New York: Philosophical Library, 1957. Pp. 653.

The publishers have given us a useful collection of Pavlov's works translated from the Russian edition. Many American readers will, however, find much of the material familiar and practically identical with parts of the Gantt and Anrep translations. The collection includes, for example, Pavlov's *Psychological Review* article entitled "Reply of a Physiologist to Psychologists" which had earlier been reprinted by Gantt in *Conditioned Reflexes and Psychiatry* (1941). (This, incidentally, is one paper with which every American psychologist should be familiar.)

This collection of Pavlov's writings is distinguished from others primarily by the inclusion of approximately seventy pages of fragments of statements from the Wednesday seminars. While the statements attributed to Pavlov suffer from the fact that they were not edited by him, they give us a clear impression of a vigorous and committed scientist. The fragments from the seminars are devoted largely to the errors of such "idealists" as Yerkes, Kohler, Sherrington, Janet, and Claparède. Pavlov emerges from these pages as a vigorous and polemical materialist but at the same time, in spite of his attacks on dualism and animism, he appears to accept a form of dualism—the kind of dualism perhaps most commonly encountered among physiologists. This dualism, while explaining mind completely in physiological terms, does not deny its existence. It seems fair to say that Pavlov had no real understanding or appreciation of the non-reductive behaviorism of American psychologists. It may, therefore, appear to American readers that Pavlov in his diatribes against the soul was wasting his time tilting against windmills. Actually Pavlov's attacks upon his colleagues do have some point. Sherrington and Kohler have been strongly influenced by metaphysical dualism. On the other hand, Pavlov, by

whole-heartedly accepting the dogma of the brain as the seat of the mind, scarcely seems to have escaped his dualistic heritage.

The fragments make interesting and easy reading because of their informal and polemical style. In fairness to Pavlov, however, it should be said that these informal statements probably fail to represent him at his critical best. Their chief value lies in what they tell us about Pavlov, the man,—about his scientific curiosity, his warmth and impulsiveness, his intolerance of sham and pretentiousness, his avid interest in the work of professional colleagues in other countries, and his endless quest for more objective analysis of the human mind.

In addition to the collection of scientific writings there is a brief autobiographical statement by Pavlov in which he reflects with modesty upon his career. In his own eyes Pavlov was a simple, unassuming man who asked little of life and felt more than amply rewarded by it. There is evidence, too, of his deep devotion to science and of his great perseverance in pursuing his intellectual goals.

Koshtoyants is the author of the 32 page introductory biographical sketch. While this biography includes many interesting details, it contains so many obvious errors that the author's authority becomes suspect. Woodworth, for example, is described by Koshtoyants as a Gestalt psychologist. Such errors and misunderstandings as well as those of several of the discussants quoted in the fragments make it clear that at least a few of the Pavlovians have read American psychologists with considerably less care than did Pavlov himself.

Denison University

PARKER E. LICHTENSTEIN

HAMMER, EMANUEL. *Creativity. An exploratory investigation of the personalities of gifted adolescent artists.* New York: Random House, 1961. Pp. 150.

While much has been written of late about creativity, we are only beginning to receive reports of significant research on the topic. The author had an unusual opportunity to study the personalities of eighteen gifted adolescent painters who happened to be studying in the Scholarship Painting Workshop of New York University. The book is essentially a report of this study including research design, case studies, and follow-up studies.

In one of the concluding chapters the author attempts to construct from his data a composite picture of the creative personality. The creative person emerges as a relatively autonomous, self-assertive being, reserved and tending to be an observer rather than a participant. He is capable of feeling deeply and is sensitive to the pulse of his emotions. While lonely and feeling rejected, he is less fearful of facing himself than others.



A final chapter examines the relationship between creativity and maladjustment. In an appendix there is a discussion of the finding of the study by Leo Rosten, Margaret Naumberg, and the author. The discussion tends to emphasize the author's use of projective techniques and provides suggestions for further study. One hopes that many of the interesting observations made by the author and discussants will receive the serious study they deserve.

Denison University

PARKER E. LICHTENSTEIN

GUDAS, FABIAN. *Extrasensory perception*. New York: Scribner's, 1961. Pp. vxiii + 141.

This small volume is more than a good collection of interesting papers on the topic of extrasensory perception. It is designed as a study guide for a controlled research paper, one in which the student finds all of his sources in one anthology. This idea is new to the reviewer and may have merit for introductory courses. A brief guide to writing a research paper is included.

Psychologists will be more interested in some of the articles included in the collection. There is a sampling of early writings, Mather, Bacon, Faraday and Huxley, for example. There is an interesting bit by Mark Twain and a brief review of the field up to 1897 by William James. Papers by Skinner, Leuba, Rhine, Soal, Broad, and Boring will be familiar to many psychologists.

Denison University

PARKER E. LICHTENSTEIN

SHIPLEY, THORNE (Ed.) *Classics in Psychology*. New York: Philosophical Library, 1961. Pp. xx-1,341. \$20.00.

Within the confines of a single volume, the scholar has a convenient reference and source book in this work. The classics included cover a publication range of 150 years and represent every major theoretical viewpoint or school of psychology and every outstanding field. Translated for the first time into English are selections from Herbert, Wertheimer and Wundt. A list of the other prominent writers included follows: Helmholtz, Mach, James, Titchener, Stern, Pinel, Esquirol, Charcot, Bleuler, Kraepelin, Rush, Prince, von Jauregg, Sakel, Jackson, Sherrington, Freud (*Studies on Hysteria*), Adler, Jung, Pavlov, Watson, Hull, Cattell, Binet & Simon, Rorschach, Aichhorn, Hail, Piaget, Köhler, Koffa, Isaac Ray, Lewin, and McDougall.

University of Wichita

N. H. PRONKO

LAWSON, REED. *Learning and behavior*. New York: Macmillan, 1960. Pp. 447.

The organization of the book is simple and meaningful. It begins with the usual introduction to the subject matter of learning, extends to interactions of simple habits, and ends with complex learning and retention (Section IV). Section III (which for the sake of continuity might have been better placed at the end of the book) is concerned with exceptions to the descriptive model used by the author to integrate the material covered. The style is quite readable with the exception of the author's predilection for introducing new terms and for referring the reader to definitions in subsequent chapters.

The general tenor of the book, although nowhere stated specifically, appears designed for an undergraduate course in learning. Viewed from the stated intentions of the author . . . "the whole book is devoted to a description of the kinds of situations in which learning can be observed to take place" (p. 29). The publisher's note asserts, "The author's aim has been . . . to summarize the important areas of knowledge that have so far been collected in the field of learning . . .". Further, the author intends to ". . . study the facts . . ." of learning, and in so doing to utilize a *descriptive* model" . . . of the observable aspects of organism-environment relationships" (p. 10). It is the opinion of the reviewer that the book falls far short of these admirable goals.

First, the model proposed is a simple S-R-X construction (X stands for reinforcement). This is quite a reasonable way to describe the empirics in learning studies. However, before long it is apparent that the model is not as descriptive as it might be nor are the concepts restricted to *observables*. Treatment of the data appears Hullian. At times it is justified as a means of better integrating the data, e.g., "habit strength" is introduced in the first chapter (p. 25) as part of the language of learning. Later on (p. 40) a distinction is made between "the external CS" and the "neural trace" in conditioning. The concepts of "inhibition" and "conditioned inhibition" are set forth to explain the facts of extinction (pp. 56-58). However, a decided disservice is done to the introductory student of learning since these two concepts are by no means universally accepted by learning theorists as necessary or even desirable aids to understanding extinction (e.g., competing response theory). Apparently the author's theoretical predilection also prevents an adequate coverage of the kinds of situations and facts of learning. As a specific illustration, in pages 184-221 concerning discrimination learning, not one reference is made to Lashley's classic works. Further, the author describes at great length (pp. 190-194) the T-maze paradigm used to explore simultaneous vs. successive discrimination learning. However, he does not mention the interesting, and in some cases, the unique contributions made by Lashley to the area

of pattern discrimination learning with the jumping-stand. Nowhere does he mention Krechevsky's (1932) important study on "hypotheses."

To choose but another example, the coverage of transposition neglects important considerations; e.g., the excellent, well-controlled study done recently by Lawrence and DeRivera (1954) gives increased perspective to the problem of relational learning. This lack of adequate survey is made more conspicuous by the author's own assertion that the transposition experiment is one of the variations in the basic transfer design "... generally believed to contain potentially important clues about the mechanisms that determine stimulus similarity" (p. 245). Similarly, the attempt to deal with "complex" learning omits such items as N.R.F. Maier's pioneer works on reasoning in humans.

In all fairness to the author, it should be mentioned that many of the discussions on concepts in learning are valuable to the beginning student (e.g., the characteristics of operant learning, pp. 69-110; adequate operational measurement of quality of reward, pp. 170-173). Also, a little more than 2/3 of the references are from the last ten years. However, these factors do not outweigh the shortcomings noted above which place a decided limitation on the use of the book as a text for an undergraduate course on learning.

Denison University

ROBERT J. SEIDEL

VETTER, GEORGE B. *Magic and religion*. New York: Philosophical Library, Inc., 1958. Pp. 555.

JOHNSON, PAUL E. *Psychology of religion*. Nashville: Abingdon Press, 1959. Pp. 304.

These works illustrate divergent trends. Vetter treats magical and religious behaviors as learning phenomena, completely explicable within psychological theory. Johnson uses the writings of various men (who with varying degrees of justice may be called "psychologists") to justify his conviction that religious behavior is necessarily central to life. Two books which resemble each other less are difficult to conceive, although they seem to agree that a minister is really some sort of psychotherapist.

Providing an overview of much of what is known concerning the evolution of religious behavior, *Magic and Religion* would be useful background reading for Social Psychology. Vetter's denial of the validity of the conventional distinction between "magic" and "religion" appears to have great value as an exercise in exorcising ethnocentrism from the student. Professionals in the field of the behavioral sciences would do well to carry out the same exercise.

*Psychology of Religion* is written for those who wish to interweave psychology and religion, and it might be useful for those whose em-

phasis is primarily upon the religious. In the realm of evaluation of evidence Johnson is on occasion guided by his religious commitment. For example, he accepts conclusions based upon approximately 50% returns from a sample of 600 atheists, but cautions the reader that Leuba's 75% return from a sample of 23,000 scientists does not include all scientists.

Denison University

PAUL T. MOUNTJOY

ANDREAS, BURTON G. *Experimental psychology*. New York: John Wiley & Sons, Inc., 1960. Pp. 595.

McGUIGAN, FRANK J. *Experimental psychology, a methodological approach*. Englewood Cliffs, N. J.: Prentice-Hall, Inc., 1960. Pp. 314.

ZIMNY, GEORGE H. *Method in experimental psychology*. New York: The Ronald Press Co., 1961. Pp. 366.

These texts raise problems concerning students' levels of ability at the beginning of courses in experimental psychology and an instructor's level of aspiration for the end of the course. All three appear to be written competently and should be useful to the instructor as well as comprehensible to the student.

Zimny's text is well suited for the first and second semesters of an elementary course with laboratory. It provides methodological material not found in the usual introductory text, and hence serves as a desirable supplement.

McGuigan's text is also methodologically oriented and written at a more sophisticated level. It would be more satisfactory for the second semester introductory student, or in the experimental course following an elementary lecture course. In this latter case, content would be supplied from other sources.

Andreas' text is suitable for advanced undergraduate majors. The first eight chapters of this text are concerned with methodology, while the last eleven are devoted to content. This dual emphasis better suits the reviewer's prejudices as to how the art of experimentation should be approached. Both Zimny and McGuigan have restricted themselves completely to method *qua* method. Andreas' chapter on social processes should be most useful in indicating to the student that *in principle* the experimental method can be applied to any behavior.

Denison University

PAUL T. MOUNTJOY

ALLPORT, G. W. *Personality and social encounter*. Boston: Beacon Press, 1960. Pp. 386.

The task of reviewing Gordon Allport's most recent book is not so much an assignment to a book as an assignment to the man. The

book itself is a series of essays, with only incidental mention of research, written between 1938 and the present. All essays appeared independently in such periodicals as the *Journal of Abnormal and Social Psychology*, as well as such unlikely places (for a psychologist) as the *Crane Review*. Allport speaks for himself in the introduction,

. . . (These essays) are neither 'technical' nor 'popular.' They have been written to amplify the theory of personality contained in my book *Personality: a psychological interpretation* (1937) or to express my concern with topical problems in social psychology.

The task Allport set for himself in these essays is a bit more grand than this mild statement of purpose would immediately indicate. Throughout the book we see a man of mixed scientific and humanistic background attempting to fit these two healthy strains into a single breed of personality theory. The reader who is so inclined can ponder for himself how far we have come since the early 17th century when these two aggressive world views first clashed head on.

The amazing, and happy thing is that Allport seems convinced that he, at least, can move freely back and forth between (in the words of C. P. Snow) "these two cultures." Witness his closing comments in the semi-introductory first essay,

Personality is not a problem for science or art exclusively, but for both together. Each approach has its merits, but both are needed for even an approximately complete study of the infinite richness of personality.

To illustrate his own versatility he goes on to enumerate these particular merits.

Although the remainder of the book steers away from any direct mention of the nature of personality theory as demanding *both* scientific and humanistic attention it is implied in almost every essay. Sequentially, these essays tend slowly from an emphasis on the personality as "consistent," "proactive," and "open," to a series of loosely related comments on personality as it is rooted in the world, that is, as it interacts with social processes and institutions.

University of Missouri

WILLIAM BENNETT

UTKIN, I. A. *Theoretical and practical problems of medicine and biology in experiments on monkeys*. New York: Pergamon Press, 1960. Pp. 276.

Experiments at the Institute for Experimental Pathology and Therapeutics at Sukhumi on the Black Sea have been conducted for 25 years now on man's close relative, the monkey. Because of their suitability

as models of human disease, the studies reported here concern the diseases of monkeys, either those produced experimentally or arising naturally. The topics range from conditioned reflexes, experimental neuroses and attempts to obtain models of hypertension and coronary insufficiency to dysentery, atherosclerosis, poliomyelitis, and measles. The approach throughout is Pavlovian, neurological, and reductionistic and the attempt is to obtain models of human diseases for the purpose of "developing effective work on the aetiology, pathogenesis, clinical aspects and treatments of such disease" (p. VII). The gulf that exists between Soviet and American workers in approach, selection of data, and results and implications is well illustrated in the 19 studies reported here.

*University of Wichita*

N. H. PRONKO

TEICHER, MORTON I. *Windigo Psychosis*. Seattle: Univer. Washington Press, 1960, Pp. 129.

Among the Algonkian-speaking Indians of Northeastern Canada the Windigo psychosis is "the intense, compulsive desire to eat human flesh," and often culminates in actual cannibalism, usually in the immediate family. Because "aversion and repugnance to cannibalism are characteristic of the culture," even though famine is common, the phenomena are regarded as evidence of sickness. The afflicted individual is believed to be possessed of the spirit of the Windigo monster, a superhuman, man-eating giant about whom there are elaborate tales.

Presentation by the American Ethnological Society supports the competency of these investigations. As in other cross-cultural approaches to psychopathology one is impressed by the relationship of behavior to the life circumstances of the people. Treatment of the Windigo setting, the Windigo belief (31 stories) and the 70 cases of psychosis is essentially descriptive and free of the "dynamic" interpretations often characteristic of anthropological excursions into personality study. The author, however, does make a special issue of the belief system as a determiner of behavior.

*Denison University*

IRVIN S. WOLF

*Soviet psychology: a symposium*. New York: Philosophical Library, 1961, Pp. 109.

A foreward by Ralph B. Winn is followed logically by an introduction by Hans Hiebsch which leads naturally to an account of the development of Soviet psychology by A. A. Smirnov. The present tasks of Soviet psychology are discussed by Leontiev and the role of heredity and the materialist theory of Posnanski. The final three chapters cover the heart of the matter. These are: the intellectual development of the child by Leontiev, problems of the child's person-

ality formation by Kostiuk and investigation of pupil personality by Shnirman. In the reviewer's opinion, this is an important book despite its small size. The most salient feature of the volume is the constant comparison and contrast that is made between Soviet psychology and Western "bourgeois" (sic) psychology. The Soviet participants show a keen sensitivity to the two-way interrelationship of Soviet psychology and the social, economic and political philosophies in which it is embedded. Nor do they hesitate to point out the effect of our capitalistic system on our concepts and procedures in psychology. This reviewer is tempted to believe that Soviet psychologists are more aware of their relation to the larger Marxist framework in which they do their work than we American psychologists are of historical, political, social and economic factors that condition our work. Their account on the importance of the state versus our stress on the individual is one striking illustration of the differences between the two systems. Small wonder that such theoretical developments as psychoanalysis, interbehavioral theory, Meyer's and Cameron's biosocial framework, and Sullivan's and Skinner's contributions, with their stress on the individual organism, found a congenial atmosphere in American democracy. This brief volume makes one appreciate how the Soviet social, economic and political conditions are allergic to our own distinctively American theoretical developments.

*University of Wichita*

N. H. PRONKO

SOLOMON, P., KUBZANSKY, P., LEIDERMAN, P., MENDELSON, J., WEXLER, D. (Eds.) *Sensory deprivation*. Cambridge: Harvard Univer. Press, 1961, Pp. 262.

This compact volume contains a collection of papers based on a symposium on sensory deprivation held at Harvard Medical School in 1958. It includes a provocative preface, which the reviewer wishes could have been expanded into a chapter, six experimental papers, three clinical papers, two chapters dealing with clinical applications, three chapters on theory, and a final chapter that gives "the sense of the meeting."

The real value of the book is not so much the research reported, which is somewhat dated and familiar to SD investigators, but rather the thoughtful and insightful reactions of the individual contributors to their research findings. Most certainly the publication is an up-to-date representative report of SD research as of 1958, but more importantly, it predicted surprisingly well the empirical and theoretical directions that SD research would take.

On the negative side, the section on applications is disappointing in quantity as well as quality. Quite frankly, Azima in his original article presented a more convincing and compelling account of the therapeutic use of SD than he does in his symposium paper. Disap-



pointing also was the insufficient discussion on methods of producing a SD state. Only a cursory reference is made to one of Lilly's most important findings, namely, that with the immersion of a subject in a water tank, even the very dramatic and deviant behavioral reactions can be produced in two to three hours of deprivation.

To sum up, what the book may lack in continuity and background material related to other areas of psychological experimentation, it makes up for in theoretical formulations, neurophysiological underpinnings, and promising suggestions for future research programs.

University of Florida

MALCOLM H. ROBERTSON

MASSERMAN, JULES H. (Ed.) *Psychoanalysis and social process*. Science and Psychoanalysis: Vol. IV. New York: Grune & Stratton, 1961. Pp. 189.

Some of the problems that have arisen from the recent attempt to develop increased communication with behavior scientists is the unifying theme of this work. The heterogeneous articles, which are represented by such distinguished social scientists as Weston LaBarre, Harold Lasswell, Milton Mazer, Marvin Opler, Talcott Parsons and John Spiegel, are organized under the following four headings: Psychoanalysis and the social order; Psychoanalysis and Transactional Dynamics; Communication and Therapy, and Psychoanalytic Training. The outstanding feature of this volume is its effective demonstration of the penetration of psychoanalytic ways of thinking into all the social sciences.

University of Wichita

N. H. PRONKO

ULETT, G. A. and GOODRICH, D. W. *A synopsis of contemporary psychiatry* (2nd ed.) St. Louis: C. V. Mosby, 1960. Pp. 297.

LOFTUS, T. A. *Meaning and methods of diagnosis in clinical psychiatry*. Philadelphia: Lea & Febiger, 1960. Pp. 153.

Admittedly eclectic, these books are not scholarly contributions in the sense of adding significantly to our knowledge; nor are they texts surveying a field. They are designed as "crutches" for the tyro psychiatrist or the psychologically unsophisticated physician. To the extent that books of this sort give comfort to such persons they should give perhaps reciprocally that much less to their patients. Beyond techniques to aid the non-specialist in knowing what he should *not* do, encouragement of this kind of "psychiatry for all" may be dangerous to both patient and profession.

Denison University

IRVIN S. WOLF

MUENSTERBERGER, WARNER and AXELRAD, SIDNEY (Eds.)  
*The psychoanalytic study of society: Vol. 1.* New York:  
 International Universities Press, Inc., 1960. Pp. 376.

This series, formerly known as *Psychoanalysis and the Social Sciences*, has been renamed as indicated above because "psychoanalytic thought and theory have broadened in a direction which makes psychoanalysis not just one of the ancillary means of studying and enriching the social sciences but which makes it possible to study the institutions of any social structure and of any culture." As in the volume reviewed above, the newer trend is for the sociologist, anthropologist, and social psychologist to employ the psychoanalytic framework in the study of their own data instead of, as formerly, tolerating the intrusion of the psychoanalyst into these areas of the social sciences. There is a wide and varied coverage of subject matters as the following partial list indicates: characteristics of totalitarianism, defense techniques in totalitarian ideology, the efficient soldier, cultural values and modal conscience, ethnopsychiatric problems, creativity and religion.

*University of Wichita*

N. H. PRONKO

Joint Commission on Mental Illness and Health. *Action for mental health.* New York: Basic Books. 1961, Pp. 338.

This report, while not exhaustive, is a highly readable, informative, and thoughtful appraisal of the needs and resources of the nation's mental health program. For example, the Commission found that close to one-half of the patients in most state hospitals receive no direct treatment for their illness. The explanation is found in the limited number of qualified personnel and in the lack of verifiable knowledge about effective treatment. On the other hand, an optimistic note is sounded in the consistently downward trend in the number of patients living in public mental hospitals. Moreover, the onset of this trend is clearly coincident with the introduction of tranquilizing drugs. However, as the report indicates, during the same period of time there has been an increase in financial support for mental health at the national, state, and local levels, more liberal discharge and parole policies, and a somewhat improved social treatment of patients returning to the community.

When the progress of mental health programs is compared to that of other types of health programs, there is no justification for any complacency about the small gains that have been made. In general, two major obstacles have slowed progress in this field. One is the manpower shortage and the other is the lack of acceptance of the mentally ill by both professional and lay people.

To develop and maintain an adequate program for the treatment

and prevention of mental illness, the Commission recommends, among other proposals, long-term basic research programs, a revised philosophy of what is treatment and who can do treatment, and a concerted effort to increase public acceptance of the mentally ill.

All in all, with the exception of the section on advice concerning matters of tax structure relevant to legislative appropriations, this is the most informed and authoritative voice to speak out on this subject in many a moon.

University of Florida

MALCOLM H. ROBERTSON

MOWRER, O. HOBART. *The crisis in psychiatry and religion*. New York: D. Van Nostrand Company, Inc., 1961, 264 pp.

A collection of documents have been here brought together, documents that are records of the author's articles and lectures before professional and lay groups. Each chapter has an introductory statement which is intended "to highlight the argument of the book as a whole and to link successive chapters together." The author criticizes Psychoanalysis as a formal structure and as a system of practice, but he also takes theology to task for finding Psychoanalysis so congenial. The implications of the book point to a much-needed reform in contemporary theology, as well as in psychiatry and clinical psychology before a synthesis can occur that will be valid for a unified conception of man and for his therapy.

University of Wichita

N. H. PRONKO

DREVER, JAMES. *Sourcebook in psychology: a course of selected reading by authorities*. New York: Philosophical Library, Inc., 1960, 335 pp.

Here goes another candidate to join the ever-increasing ranks of books containing selected classics (or portions thereof) from various areas and periods of psychology. The sources in Drever's volume are organized into three parts, namely "The Study of Behavior," "The Maturing Mind" and "The Study of Personality." An "Introductory Reading Guide" by the author provides a perspective for the whole, while transitional comments are meant to integrate the separate items.

Biographical notes on the authors quoted are included. The following names of authors picked at random reflect the contents of this volume: Bain, Boring, Burt, Cameron (Norman), Flugel, Freud, Haeckel, James, Jung, Mowrer, Rivers, Spencer, Vernon, Watson, and N. A. B. Wilson.

As a volume for review, Drever's *Sourcebook* is somewhat neutral. One can hardly say more than that such a book is useful in introducing the early student of psychology to men whose contributions shaped contemporary psychology.

University of Wichita

N. H. PRONKO

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